THE LOGIC OF MODERN PSYCHOLOGY

BY THE SAME AUTHOR

THE MEANING OF MUSIC, 1931

The Logic of Modern Psychology

BY

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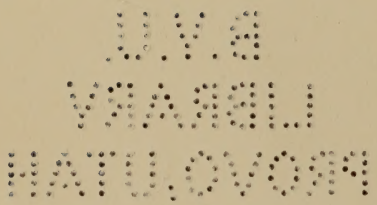

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Preface

PSYCHOLOGISTS OF LABORATORY and brass instrument persuasion have frequently sought to maintain the purity and rigor of their scientific status by proclamations of emancipation from all contacts with philosophy. Yet here am I, bred in the tradition of experimental psychology, writing a book, albeit a small one, let it be noted in its favor, which shows that I am a traitor to the cause, for the book deals only with words and concepts, not at all with material facts and empirical data.

Defection of psychologists from the ideal of psychology as a pure science has been common enough during the relatively short period, hardly more than half a century, in which this ideal has been in force. The ways of transgression have been most frequently either toward philosophy, university administration, or premature application—as may be seen by the large number of pseudophilosophical books, including the present volume, written by psychologists, by the disproportionate number of chairmen, committee men, deans, and presidents who were at one time professors of psychology, and by the feverish haste with which scattered facts and dubious theory are tortured and rushed into practical service.

The reasons for defection, as given by those who remain loyal, are said to be intellectual senility, which leads to phi-

losophy, or disillusionment, which leads from the laboratory into offices of administration, or cerebral myopia, which causes the products of psychological research to appear as though they were ready for the market-place. The last two forms of disloyalty have no besmirching influence on the contents of this book; but if concern for the logic of psychological definitions and methods reveals for philosophy a love which is more than platonic, then I must plead guilty and seek for some defense of this love, even if it is not my first and only true love.

The need for insisting upon rigid separation of philosophy and psychology no longer exists. Psychology has struggled through to a flourishing state of complete official independence, and has become an arrogant and troublesome discipline, so that most philosophers instead of trying to keep protecting hands on their former restless partner are only too glad to be rid of him. In the early days, while the battle for independence was still on, fraternization with the enemy was rank disloyalty; but now that philosophy has been defeated in its effort to hold on to the domain of psychology, many good things may be said about the enemy which could not have been uttered during the course of battle.

That branch of philosophy which most nearly concerns psychology, as well as other sciences, is logic, especially the logic of scientific method. Brute facts that are independent of the ways and means by which they are observed do not exist, or at all events, can never be known. All facts are a function of their method, so that the very first step of scientific inquiry needs the guidance of logic. In the newer fields of psychological inquiry, such as the study of personality in relation to complex social phenomena, there is an acute need

for logical analyses of methods of observation in order to arrive at some agreement as to what selections of data, out of the hundreds of thousands of items that are potentially available, deserve the name of facts. In and of themselves, however, facts are of relative unimportance, no matter how excellent the methods by which they are gathered.

The most important function of science is the construction of theory, for theories give meaning to facts and also make it possible to know what further facts are necessary, and where and how to look for them. But as soon as science applies facts to the construction of theories, it forsakes the safe ground of empirical evidence for the more hazardous realm of speculation. Some theories are good and some are bad, and the difference may be due not to the relative amounts of factual materials which they possess, but rather to the kind of logic used in their construction. In some instances the difference may be traced to concealed metaphysical assumptions which the makers of theories have more or less unconsciously allowed to creep into their speculations. Their presence, especially in recent controversies over the relative merits of various schools of psychology, has raised blinding clouds of verbal dust which might have been laid earlier by steadfast effort to give every concept an operational meaning whenever possible.

In spite of official separation from philosophy, psychology is still burdened with many philosophical concepts, partly because the philosopher still assumes that he may speak as one having authority in psychology, and because his words can not, or should not, be ignored. Ignorance is a poor protection against the officiousness of philosophers. Better by far to meet the philosopher on his own ground, armed with the ex-

plosives of empirical fact as well as with the philosopher's own armor of logic.

Yet if the psychologist enters the domain of philosophy, his officious manner must in turn arouse righteous indignation among philosophers. I have been only too well aware of this likelihood, and have debated with myself for some time whether it would not be wiser, no matter what domain the contents of this book might fall into, at least to avoid the word "logic" in the title. For better or worse, I am a mere psychologist, not a logician; but since this book deals more with logic than with psychology, I have finally decided to let the title say so. If it is not logic, or if it is bad logic, I can only beg the philosophers and logicians who may happen to read it to forgive my presumption, or at least to direct their wrath toward me, not toward psychology in general. I know that this book will offend many psychologists. But I rather enjoy that prospect, for if the book helps to renew an interest in the basic assumptions of psychology, it will have had some small share in expanding what strikes me as the most important, but by no means the most vigorous part of psychology, at least in this country, namely, Theoretical Psychology.

The main theses of the book are as follows. (1) Psychology is supposed to be the study of mind, but since no one knows what is meant by mind, it is impossible to define psychology. No operational criteria have been discovered by which so-called mental and physical events can be distinguished. The nature of all events is ontologically the same. Psychology is a convenient division of scientific labor, not a subject-matter.

(2) Classical psychology and behaviorism, and every other

possible school or point of view, have the same subject-matter. All efforts to restrict their scope of inquiry because their data are direct experience, introspective, subjective, objective, biological, physiological, material, physical, mental, or what not, turn out upon critical examination to be meaningless.

(3) There are two portraits of human nature: one given by intuition and direct description, the other made by systematic and experimental analysis of the conditions correlated with the events initially described. The two portraits do not agree, for they are not the same thing. The first portrait is merely a point of departure. The second portrait is the major concern of psychology. If modern psychology therefore presents an account of human nature which is at variance with that of common sense and intuition, the trouble is not with psychology, but with those whose adverse criticisms of modern psychology on this score betray such deep misunderstanding of the logic of science.

(4) Exact definitions of psychological concepts are best given by functional equations. On one side of the equation stand experimental observations and operations; on the other, a word or phrase designed to comprehend them. Departures from this restriction may occasionally be useful and even necessary, but are fraught with danger. The danger can be minimized by determining the extent of departure from the initial definition by equation; by knowing and admitting that such departures are scientifically vague and operationally meaningless, however stimulating they may be for research and discussion.

(5) The principal task of theoretical psychology in making a scientific portrait of human nature is to discover the

immediate antecedents of initial descriptive data. These antecedent conditions are located within the biological organism. All psychological explanation must therefore move in the direction of physiology. The theoretical importance of psychological descriptions, as contrasted with any practical significance they may conceivably possess, derives almost exclusively from the light they throw on physiological mechanisms.

(6) If physiological knowledge is not available, the construction of psychophysiological hypotheses becomes necessary. Yet the making of such hypotheses entails an inevitable circularity: the nature of the hypothesis, or explanation, is determined logically by the nature of the descriptive events which the explanation is supposed to account for. But since all 'correct' scientific explanations turn out to be nothing more than statements of observed correlations, *i.e.,* statements of what is already known, the circularity of psychophysiology is no worse nor better than that of any other science. Proper scientific questions are always *what* and *how,* never *why.*

(7) Since so many of the physiological conditions which underlie psychological data are still inaccessible, the correlations or laws of psychology are incomplete, crudely empirical. Their probability-value is low, their theoretical interpretation unproved.

(8) Sound theory is necessary before extension and application of knowledge to practical ends can be made with assurance of successful results. Where the need is great, where it is better to do something than to do nothing at all, as in the case of mental disorders, it may be justifiable to make use, with all possible precautions, of whatever psychological knowledge is available; in all other cases the best service psy-

chology can render mankind is to make frank confession of the present tentative and insecure nature of all conclusions regarding the laws of human nature. For a long time to come psychology should remain in the laboratory and library. If the general public is not fully aware of what is going on there, so much the better.

The above ideas, as elaborated in the following pages, may be called Critical Positivism, if anyone cares to attach a label to them. They have their origin in various sources, some of which are obvious to anyone who has followed recent developments in science and philosophy, while others are elusive and difficult to trace. I should be happy to give credit where credit is due in some more appreciative manner than by the occasional use of coldly impersonal footnotes and references inserted into the text; but I hesitate for fear that more directly personal expressions of gratitude and indebtedness would not be welcomed by those to whom they might be directed. For all that I can tell, this book may come to be judged a very bad one, in which case any reference to people who I think have influenced my point of view would be an unkindness. I have been told by philosophers and psychologists who are more competent and informed than I am that my ideas are all wrong, so that misgivings as to the value of these ideas are shared by others as well as by myself. I can not refrain, however, from mentioning one or two names.

In spite of occasional flat disagreements which I have expressed at one time or another, I have been greatly influenced by the writings and accomplishments of the *Gestalt* psychologists, especially those of Professor Köhler. They have always impressed me as important, and for that reason have helped

me by making it necessary to find out why I could not always agree with them. If there is any consistency in my thinking about matters which pertain to psychology, I should like to feel that Professor Köhler is in some measure responsible, and that he will not be offended by my belief that certain of the ideas in this book were inspired by him.

The academic environment is supposed to be alive with ideas, and to encourage discussions and verbal clarifications. Unfortunately I have not often found it so, perhaps largely through some fault of my own. Routine matters of administration, endless talk about politics, and gossip about promotions and salaries leave too little time for profitable exchange of scientific knowledge and opinions among colleagues. The one exception to this rule has been, for me, the source of the best intellectual stimulation I have gotten outside of books. I have spent hours of heated argument about psychology with my friend and former colleague, Dr. J. G. Beebe-Center, during which all questions relating to the outside world of practical affairs were pleasantly forgotten. If any of the ideas in this book have value, I am sure they reveal in part the results of our discussions, and that it was not I to whom the ideas first occurred. In fact, this book represents my understanding of a point of view which Dr. Beebe-Center and I often tried to elaborate, but since my understanding is a poor medium for the transmission of thoughts, I dare not credit him with ideas which he might fail to recognize or refuse to own in their present formulation.

My habits of writing are such that my first draft is also the final product, so far as I am concerned, and represents the only ability or interest I have in this direction. I was therefore glad to have the generous help of my critical wife

who in the interest of clarification and simplification insisted upon many changes in the text.

The thanks of the printer, as well as my own, for receiving a neat and nearly faultless manuscript in place of my original hunt-and-punch typing or illegible scribbling, both full of bizarre deviations from Webster, are due to Mrs. Frances K. Withington and Miss Irene M. Varga.

Permission to quote brief excerpts from their publications has been secured from the American Psychological Association, Columbia University Press, Harcourt, Brace and Company, Henry Holt and Company, The Journal Press, Liveright Publishing Corporation, The Macmillan Company, Charles Scribner's Sons, and Yale University Press.

C. C. P.

October, 1938
New Brunswick, New Jersey

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THE LOGIC OF MODERN PSYCHOLOGY

CHAPTER I

The Demise of Mind

EVER SINCE PSYCHOLOGY made the effort, some sixty years ago, to establish itself as an independent discipline, it has been accumulating more and more divergent definitions of its subject-matter, until at the present time it is handicapped and embarrassed by the reproach that it does not know what it is trying to study or do. To the merriment of its detractors among the older sciences and to the discomfort of its own supporters, it finds it has nearly as many systems of thought as it has systematic thinkers. The much quoted words of William James, written over forty years ago, still contain a disturbing element of truth:

> When, then, we talk of 'psychology as a natural science,' we must not assume that that means a sort of psychology that stands at last on solid ground. It means just the reverse; it means a psychology particularly fragile, and into which the waters of metaphysical criticism leak at every joint, a psychology all of whose elementary assumptions and data must be reconsidered in wider connections and translated into other terms. It is, in short, a phrase of diffidence, and not of arrogance; and it is indeed strange to hear people talk triumphantly of 'the New Psychology,' and write 'Histories of Psychology,' when into the real elements and forces which the word covers not the first glimpse of clear insight exists. A string of raw facts; a little gossip and wrangle about opinions; a little classification and generalization on the mere descriptive level; a strong prejudice that

we have states of mind, and that our brain conditions them . . . This is no science, it is only the hope of a science.[1]

During the last few years there has been less wrangling over the respective merits of various schools. But the rate of change for the better is retarded by the absence of that dogmatism and intolerance which cracked the intellectual whips of these same schools when they were at their fighting best. An anaemic eclecticism which wearily consents to accept all things both good and bad may furnish sufficient drive for the collection of facts, but it also dulls the edge of critical interpretation. Not mere facts, of which psychology has more already than it can properly handle, but the meaning of facts is what pushes science ahead. Newton was surely not the first person to be hit by a falling apple, but the incident signified more to him than it had done to anyone else. Furthermore the present inclination among many psychologists to agree not to disagree can hardly last long. Beneath the surface the old prejudices persist, and the more frequently they are brought to light and aired out, the greater the chance that their dissipation will be actual rather than apparent.

These prejudices, whenever they flare up, center primarily around matters of definition. Any science which claims an independent status for itself must presumably have some characteristic which differentiates it from other disciplines. Especially should this be true of psychology, for it purports to study mind, whereas the older sciences deal with various forms of matter. The latter differ among themselves for the most part merely in methodology, but psychology differs from them all in the uniqueness of its subject-matter. The thesis to be elaborated in this first chapter maintains that all

[1] W. James, *Psychology: Briefer Course,* 1892, 467 f.

attempts to define psychology in terms of subject-matter are bound to fail, for the simple but momentous reason that all sciences have the same subject-matter.

The meaning of the word 'subject-matter' must itself be examined critically in a later section. For the moment let it be understood that it refers to the initial data of observation on the basis of which scientific conclusions and generalizations are made. For example, the belief in the continuity of biological development is based on similarities of organization through increasing stages of complexity of animal structure. Uniform differences in colors and brightnesses furnish the data for physiological theories of visual contrast. Certain astronomical measurements come from the noting of changes in size and location of spots on photographic plates. Movements of a needle-point on a dial indicate the magnitude of electrical charge in a battery. Theories of personality may be tested by comparing automobile speeds near a stop-light at a lonely intersection late at night with and without a policeman in sight. All of these observations are visual. Other sense departments may be used, and in psychology very frequently are, but visual process tends to be favored because of the greater accuracy with which it can be reported.

MENTAL AND PHYSICAL CATEGORIES

Difficulties with definitions of psychology in terms of subject-matter all derive from the fact that no philosopher or psychologist has ever been able to demonstrate to the complete satisfaction of more than a limited number of followers that the events and objects of this world can be unequivocally divided into two basically different categories, mental and physical. What one philosopher calls mental, another

will insist is physical. A third philosopher will contend that both classes are really physical, while a fourth argues that all events are fundamentally mental. To which a fifth quite reasonably adds that they are probably neither. No digression, however brief, into the history of metaphysics is required in support of the contention that disagreement among philosophers in such matters is utter and complete. It is common knowledge that neither idealism, whether objective or subjective, nor realism, whether critical or not, nor yet materialism, whether crude or refined, nor any other possible view regarding the nature of the world in which we live has ever gained anything like general acceptance. And yet psychology can not completely ignore the question, for if psychology is the study of the mind, some criterion must be found by which it is possible to know when mental phenomena are under observation. To any psychologist, however, even if he has only a smattering of philosophical training, it should be abundantly clear, although unfortunately it actually is not, that to try to differentiate psychology from other sciences by finding for it a unique set of mental events will only serve to augment metaphysical confusions.

For purposes of scientific definition, naïve inferences from common everyday observation can not be trusted. Distinctions are too easily and uncritically drawn. The stone kicked off the sidewalk is a physical object, whereas the recollection the next day of having kicked it off is mental. It is as easy as that to divide the world of common sense into two parts, but a moment's reflection brings up metaphysical doubts. How does one know that he kicked a stone? Why, it was both seen and felt. Visual and tactual processes, however, are sensory, and as such, according to one tradition of long and re-

spectable standing, they belong to psychology. At any rate, the characteristics of these processes are partially determined by the nature of a human biological organism, so that to jump from them to the notion of an independent physical stone involves a logically dangerous leap. The memory of the stone, on the other hand, is usually had by way of visual or tactual imagery, or both—or possibly by verbal kinaesthetic symbolization. These imaginal processes are again sensory, but sensory data are used by astronomers, physicists, and chemists in their own fields of observation. If such data constitute the initial objects of inquiry in the physical sciences, it is not clear how the same data can also be regarded as basic to the mental sciences.

CLASSICAL PSYCHOLOGY

In 1874, when Wundt was trying to secure for psychology an independent status among the sciences, there were two diametrically opposite views regarding the disposition of sensory phenomena. These phenomena have always been of major concern for physiologists, psychologists, and also physicists. In certain branches of philosophical writing, especially the British empiricists, they have frequently been regarded as the origin of all mental life, the initial impressions on the *tabula rasa,* the "givens" of experience. Wundt tried to define psychology in terms of *immediate* experience, as contrasted with the mediate experience of the physical sciences. The distinction has all but disappeared today. It is enough to remember that Wundt was dealing primarily with sensory phenomena. His general approach to the systematic problems of psychology can be schematically illustrated under four headings:

1. Subject-matter of psychology	Consciousness
2. Method of psychology	Experimental observation and analysis
3. Results of analysis (elements)	Sensations
4. Principle of synthesis	Associationism

In keeping with the climate of opinion which prevailed at the time, Wundt maintained (1) that the phenomena of consciousness—ideas, thoughts, imagination, volition, feeling, emotion, *etc.*—constituted the proper field for psychological inquiry. The philosophical style of speculation in the armchair must be replaced, however, (2) by the rigorous experimental methods which the older sciences had found so fruitful. And just as the physicist had not dealt directly with earthquakes and cyclones, so the psychologist must not expect to cope with a full-blown consciousness, but must (3) reduce it to the scope of the laboratory and split it up into its parts or elements. These elements turned out to be sensory in nature, and were regarded as the irreducible constituents of all mental life. (4) The principle of associationism, as an explanation of spatial and temporal coherence, Wundt took over directly from the British empiricists.

During the same year, 1874, in which *Die Grundzüge der physiologischen Psychologie* appeared, Brentano published a small volume, *Psychologie vom empirischen Standpunkt,* in which he drew a sharp distinction between sensory phenomena, or *contents,* as he called them, and psychical *acts.* Brentano derived from the scholastic tradition in philosophy and, in spite of the title of his book, was not nearly so concerned as Wundt was with what we should now call empirical evi-

dence. Subtlety of argument was his court of appeal. The mind possesses, Brentano believed, certain capacities which are revealed to introspection as acts, like judging, discriminating, comparing, relating, *etc.* In judging weights, for example, the tactual and kinaesthetic sensory processes are contents toward which the act of judging is directed. The latter is psychical, the contents are physical. It may be noted incidentally that although Brentano's system was at first overshadowed by its rival, it is in many ways more in keeping with modern psychology than is the point of view developed by Wundt. The important fact for the present argument is that at the very outset of modern psychology two men deeply concerned with the problem of systematic definition were at irreconcilable odds with each other over the question of subject-matter. Wundt regarded sensory phenomena as the foundation-stones of the new science. Brentano ruled them out of psychology entirely and assigned them to physics. The same set of events was called mental by one, physical by the other.

Two of Wundt's students, Titchener and Külpe, shifted the emphasis in their systematic writings from subject-matter *per se* to point of view toward subject-matter. Psychology is the study of experience *regarded as dependent upon an experiencing individual.* Both psychology and physics start from the same world of experience, but physics treats its observations as though they were independent of the person reporting them, whereas psychological observations are interpreted as functions of a living organism. The change was potentially a significant one, for it placed the burden of explanation on the properties of the physico-chemical organism in which the conditions for experience reside. Neither Külpe nor Titch-

ener, however, realized the full implications of the change. Külpe saw much more clearly than Titchener did the limitations of Wundt's psychology, and toward the end of his life adopted a position more nearly resembling that of Brentano; but at the same time he hastened the revolt of behaviorism by excessive use of a dubious style of introspection, and introduced subtle epistemological distinctions between psychic reals and conscious actuals which unfortunately have not wholly disappeared from psychology today. Titchener paid little more than lip service to the nature of the experiencing individual. For him subject-matter was all important, and he drove his analysis of it almost to the vanishing point of refinement. In one of his last publications he could write:

> We have, I think without question, passed from infancy into childhood. Our independence of physiology is a guarantee of that: we no longer feel any necessity of consulting physiology when we lay out our investigations; we do not necessarily borrow physiological apparatus and procedures; it does not occur to us to imitate physiology in the presentation of results; in a word, we are out of our physiological leading-strings. It is pleasant and reassuring, certainly, if while an enquiry is in course or after it has been brought to completion we can make a cross-correlation with physiology; but we feel ourselves, none the less, to be independent; we do not lean upon physiology.[2]

With respect to the question of subject-matter, all of the early attempts at critical formulation of the field of scientific psychology remained close to the convictions of common sense and to a tradition of long standing in philosophy, namely, that a definition of mind must in some way comprehend the phenomena of consciousness. However much the points of view may have differed in detail, they all sub-

[2] E. B. Titchener, Experimental psychology: a retrospect, *American Journal of Psychology,* 1925, 36, 322 f.

scribed to that one article of faith. Even Brentano and Wundt, in spite of their sharp differences of opinion, were both trying to discover unassailable criteria of consciousness. It was therefore not a new outlook upon mind, as far as subject-matter was concerned, which came into existence during the middle of the last century. It was merely an attempt to crystallize, within the framework of nineteenth century science, an attitude which was prevalent not only at the time, but which had characterized philosophical psychology from the very beginning. The variations on this type of definition, whether in the history of philosophy or in the psychology of the latter half of the last century, may be referred to, primarily for the sake of convenience, as *classical* psychology—taking Wundt's system as a typical example.

BEHAVIORISM

Movements away from definitions of psychology in terms of conscious phenomena were brought to systematic formulation around 1914. The word "behaviorism" is inextricably linked with Watson's dogmatic revolt, but the ground had been prepared long before. Men who in their early writings had more or less taken it for granted that consciousness was the subject-matter of psychology were giving expression during the first years of the present century to occasional doubts. In biology and physiology, where the concept of consciousness played a negligible role, numerous investigations indistinguishable from what would now be called animal psychology were being pursued. James wrote in 1904: "For twenty years past I have mistrusted 'consciousness' as an entity; for seven or eight years past I have suggested its non-existence to my students, and tried to give them its pragmatic equiva-

lent in realities of experience. It seems to me that the hour is ripe for it to be openly and universally discarded." [3] One year later Jennings published his elaborate studies of the behavior of lower organisms in which he was led to argue that all so-called psychic states, like perception, choice, attention, desire, emotion, foresight, *etc.,* could be equated to, and certainly most effectively examined, in the external reactions of an organism; that the question whether consciousness existed furnished little more than idle speculation, for consciousness could never be known directly, but only through outward manifestation; that if the word had any meaning at all it must refer to an hypothetical adaptive function by means of which differential response to an environment was made possible.[4] It only remained for Watson to unite these two tendencies into a system of psychology which refused to accept consciousness as its subject-matter. Here at last was a psychology which could take its place in the company of natural sciences. It proposed a new definition, again in terms of subject-matter, but a subject-matter which was real, objective, physical.

Human psychology has failed to make good its claim as a natural science. Due to a mistaken notion that its fields of facts are conscious phenomena and that introspection is the only direct method of ascertaining these facts, it has enmeshed itself in a series of speculative questions which, while fundamental to its present tenets, are not open to experimental treatment . . . Psychology, as the behaviorist views it, is a purely objective, experimental branch of natural science which needs introspection as little as do the sciences of chemistry and physics . . . It can dispense with consciousness in a psychologi-

[3] W. James, Does 'consciousness' exist? *Journal of Philosophy, Psychology and Scientific Methods,* 1904, 1, 477 f.

[4] H. S. Jennings, *Behavior of the Lower Organisms,* 1905, 328-337.

cal sense . . . This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier which exists between psychology and the other sciences. The findings of psychology become the functional correlates of structure and lend themselves to explanation in physico-chemical terms.[5]

The controversies to which behaviorism gave rise are too fresh in memory to need repetition. The classical psychologists argued that the movements of an organism are physical events dealt with by the biological division of physical science, and that although they may be useful as auxiliary evidence, they are not such stuff, for example, as dreams are made on. Images and ideas and dreams do not spread themselves out for inspection in the external environment, nevertheless they are the very essence of what is meant by consciousness. The retort of the behaviorists, if not courteous, was at least difficult to answer, and classical psychology in this country found itself waging a losing battle. In the heat of the vitriolic explosion of words, the similarities of the two points of view were lost sight of, and the differences magnified beyond all reason. It has taken time, two decades in fact, to view the two systems in proper perspective and to discover that Watson's formulation differed in no essential detail from that given by Wundt, except in the apparent difference in choice of subject-matter. Watson's general approach to the systematic problems of psychology can be schematically illustrated under four headings:

1. Subject-matter of psychology	Behavior
2. Method of psychology	Experimental observation and analysis

[5] J. B. Watson, *Behavior,* 1914, 26-28.

3. Results of analysis (elements)	Reflexes
4. Principle of synthesis	Conditioning

The analysis of gross forms of behavior into constituent reflexes parallels closely the resolution of complex states of consciousness into sensations. The difference lies solely in the name given the objects of analysis. The procedures by which the reduction to simpler processes is brought about are in both instances identical. The principle of synthesis is in both systems practically the same—the difference is primarily one of words. The general law of association states that when two ideas have been repeated together, the recurrence of one is likely to produce the other. The repeated presentation of two stimuli in such a way that one of them produces a response originally elicited only by the other constitutes the factual basis on which the principle of conditioning has been built.

In more recent years critical behaviorism, in its defense of subject-matter, has been beset by two insecurities—insecurities which are precisely analogous to those which overtook the later stages of classical psychology. One of them is metaphysical, the other logical.

SENSATION AND REFLEX

However real and objective and physical an event may appear to be, philosophy can produce convincing and embarrassing arguments to the effect that things are not what they seem. The rat running in the maze may look like an independent physical object. The red on the color-mixer may be regarded as dependent mental substance. Whether such easy assignment to separate categories is correct or not can hardly be decided on the basis of any operational criterion yet agreed

upon. Convictions in such matters run deep into the temperaments of men, and their supporting arguments fulfill a wish, and consequently flow from an inner need rather than from any coercive premises furnished by the properties of the events under dispute. At any rate, philosophers can argue the matter effectively either way, which should have persuaded psychologists long ago that such arguments lead nowhere. The belief in the real independent existence of the rat is a leap of animal faith. The belief in the subjectivity of red is equally insecure. We have already seen that Brentano regarded color as physical.

All that one has to go on initially in any scientific work is a protocol in which reference is made to an event of some kind which someone has observed, be he behaviorist, introspectionist, chemist, or astronomer. The ultimate nature of this event is irrelevant, and may remain forever unknown, as far as science is concerned. For it is not the event *per se* which yields the most significant material for the construction of scientific concepts, but, as will be illustrated more fully later, the relations into which the event enters. An unadorned description of an event, without any reference whatever to the conditions under which the descriptive observation was made, is no decent scientific fact. The event begins to secure factual status only when given a relational setting. In the protocols the events receive mention in order to make clearer the relationships of which the events are the terms. It may be the business of metaphysics to try to come to grips with the fundamental nature of events, but for science these events, whatever their nature may be, are merely the means by which the validity of relationships and concepts is tested.

If no valid distinction can be drawn between the nature of

events chosen for subject-matter by behaviorism and the experiences described in the protocols of classical psychology, it must follow that the differences between the two opposing schools of psychology, even with respect to subject-matter, are more apparent than real. All sciences begin and remain in the same world of experience, since there is none other to which safe flight can be taken. And the events of this world, so far as we are yet able to discover, can be ordered only with respect to differences in complexity and in immediacy of report. They differ not at all in kind. Lines of division merely serve the purpose of linguistic communication and turn out upon critical examination to be superficial, if not utterly meaningless. The most elusive of all lines of division, although it will always be a useful one in conversation, is that which tries to separate the physical from the mental. Behaviorism, together with every other kind of psychology which adheres to scientific methodology, is one branch of general science. But from the very beginning, the protocols of general science have contained reference to sensory materials—those very materials which classical psychology sought to appropriate as a unique basis for all mental phenomena. At the turn of the century the classical psychologists had begun to waver. Today it would be an impervious behaviorist indeed who would feel no insecurity in defending the proposition that the movement of a muscle is more 'physical' than the movement of moving pictures.

The second insecurity of present-day behaviorism, when it tries to set itself apart from other types of psychology, is a logical one. The same difficulty had been run into by classical psychology in its attempt to give systematic formulation to the concept of sensation. It was customary to define sensa-

tion in terms of its attributes, which, in turn, were regarded as inseparably connected to give the full sensation. A visual sensation, for example, without color, or without intensity, or without extensity, was deemed an impossibility. The sensation was the sum total of its attributes. In Külpe's famous abstraction experiments,[6] however, it appeared that intent to observe one attribute precluded report on the others. The attributes were not inseparably united. In singling out one, the *Aufgabe* excluded the others. Titchener, in his reply to Rahn's criticism of sensation as a mental element, argued therefore that sensation is a *concept* built up from separate observations which are logically interrelated by way of their mutual dependence upon the same stimulus.[7] The datum reported in a single observation rarely occurs in complete isolation, nor do the several data from different observations ever offer themselves together for inspection at one and the same time. Nevertheless it is possible to define sensation logically in terms of the operations performed in arriving at stateable relations between the controlled aspects of the stimulus and the correlative reports. Such sensations are not given in nature. They are created by the rules of scientific logic, and are thus several degrees removed from the pure sensory essence or "raw feel" which an earlier and more naïve psychology thought it was dealing with. So far removed are they, in point of fact, that the question as to whether any psy-

[6] O. Külpe, Versuche über Abstraktion, *I. Kongress für experimentelle Psychologie,* 1904, 56-68. See also E. G. Boring's report of an experiment by Yokoyama, *American Journal of Psychology,* 1924, 35, 301 f.

[7] C. Rahn, The relation of sensation to other categories in contemporary psychology, *Psychological Monographs,* 1913, 16, 76-85; E. B. Titchener, Sensation and system, *American Journal of Psychology,* 1915, 26, 258-267.

chology ever worked with clean sensory material becomes almost unanswerable. Psychology deals with concepts into which sensory data have somehow entered as interrelated terms. Its serious business begins at the level of the concepts, not at the level of the materials upon which the concepts are based. The materials are merely means to an end.

The situation in behaviorism today is strikingly analogous. The reflex for many years was regarded as a distinct behavioral entity which by skillful technique could be isolated and observed as a raw datum. Lists of pure reflexes were drawn up, only to be impeached and superseded by still more immaculate lists. Eyewinks, kneejerks, snores, coughs, groans, shudders, and flexions: definition by enumeration was supposed to bring out the common feature in the motley array. And to settle the matter, by removing it beyond the range of anything tangible enough to permit of sensible dispute, the reflex-arc was offered as material proof that there must be real reflexes. Not in despair, but in recognition of the way in which all scientific concepts are made, the notion of a reflex as something independent of its definition is now on the way out. A reflex is a definition, given in terms of the number and kind of analytical procedures required to make possible a statement of the functional dependence of common response-features upon stateable stimulus-conditions.

The question then arises: *what is the flexion reflex?* If we try to answer by describing in detail a stimulus and a response, we meet embarrassing difficulties. We find that the exact degree and direction of flexion may vary with many factors. We find, for example, that it was very important for our original measurements that the torso of the animal had a particular position, that the contralateral leg was, say, unsupported, and so on. But we cannot specify these

incidental conditions in our description without destroying its generality. Thereupon we shall probably resort to surgical methods. Theoretically, at least, we may pare down the structures underlying the flexion reflex until the collateral variables are no longer effective. But we can never be sure that the reflex that we have thus carved out of the behavior of the organism would not have been grossly otherwise if our operative procedure had been different. We are not sure, that is to say, that what turns up at the end of our process of isolation is the flexion reflex. There is another method open to us. In the flexion reflex we are dealing essentially with a group of correlations showing many characteristics in common. They involve the same effectors acting roughly in the same way and stimuli which resemble each other at least in their gross anatomical reference. We may, therefore, if we wish, *construct* a flexion reflex by a statistical treatment of many of these separate correlations. We may, in other words, determine and state a correlation between the characteristics common to all our observed responses and the characteristics common to all our observed stimuli, and we may name this construct the flexion reflex. But the resulting description of this statistical entity will likewise depend upon our choice of observations and upon our method of analysis.

We have been proceeding, of course, upon an unnecessary assumption, namely, that there *is* a flexion reflex, which exists independently of our observations, and which our observations approximate. Such an assumption is wholly gratuitous, but it is remarkably insistent. It arises in part from the nature of the reflex. If we remain at the level of our observations, we must recognize a reflex as a correlation. But the immediate uncritical reaction to a definition on that basis is that a correlation, in point of satisfaction, is not enough. There is an urge towards solidification, clearly evident throughout the history. We turn instantly to the reflex arc for material support. Although our knowledge of the critical part of the arc is, as we have seen, derived wholly from the observation of a correlation, we much prefer to regard the characteristics of the correlation as properties of the synapse than to retain them simply as characteristics of a correla-

tion. Under the same pressure, then, but with less justification, we are led to assume that there are isolated reflexes concealed in the behavior of an organism, which by proper investigatory methods we may discover, and in the description of behavior to state the corollary of this proposition, namely, that behavior is the sum or the integration of these units.[8]

It is plain that the reflex has become a concept, the validity of which is tested by experimental observations of the movements made by an organism. But the movements and the concept are not one and the same thing. The concept includes reference to the variations of movement and the common features running through the variations, a precise statement of stimulus-conditions which served as independent variables, the number of observations made and the apparatus used to make the observations more exact, the state of the organism during the course of the experiment, *etc.* The concept, in other words, is statistical, analytical, operational, logical, whereas it does not make sense to say that the real reflex in nature, which exists prior to any scientific definition given to it, is or is not an affair of statistics, analysis, operations, and logic. Whether, as a matter of fact, there is such a thing as a flexion reflex is a question to which there is no unambiguous answer. There are behavioral concepts into which flexion movements have somehow entered as initial terms, but the science of behavior is developed out of concepts, not out of movements of muscles. The latter are merely means to an end.

The similarity between the formation of such a concept of reflex and the way in which the concept of sensation was

[8] B. F. Skinner, The concept of the reflex in the description of behavior, *The Journal of General Psychology,* 1931, 5, 449 f.

developed is obvious. The difficulty of trying to discover whether the initial materials which have entered into the making of these concepts are fundamentally the same or different thus becomes well-nigh insuperable—both on philosophical and on logical grounds.

MIND AND BODY

One incidental advantage of considerable moment would accrue to psychology if it recognized the unity of materials which go into the building of scientific concepts. The body-mind problem would completely disappear, along with all the fruitless discussions of parallelism, interactionism, double aspects, *etc.* With respect to these issues the present view will cut no Gordian knot. It simply denies dogmatically that any knot exists. The world of mind, the world of matter, and the relation between the two: these phrases, so ambiguous, so impossible to define, have raised up blinding clouds of verbal dust. The tomes written about them may be good literature, but they are certainly bad science. Their upshot, so far as clarification of the problem is concerned, has been *nil.*

> Why, all the Saints and Sages who discuss'd
> Of the two Worlds so wisely—they are thrust
> Like foolish Prophets forth; their Words to Scorn
> Are scatter'd, and their Mouths are stopt with Dust.
>
> Myself when young did eagerly frequent
> Doctor and Saint, and heard great argument
> About it and about: but evermore
> Came out by the same door where in I went.

Psychology, although still a young science, is getting along in years. It can not afford to waste its hour "in the vain pur-

suit of this and that endeavor and dispute." The way out is simple. *If it is impossible to tell the difference between the mental and physical, then the problem of the relation between them need never arise.*

Until some valid criterion is made available for marking the physical off from the mental, no form of metaphysical dualism can be given any scientific meaning. Since centuries of effort have failed to produce a criterion, the probability that the near future will do so may be dismissed. All sciences are therefore compelled to work within a monism. Yet paradoxically enough, there are monisms and monisms, and the kind which any scientist adopts will probably be largely a matter of individual temperament. At least four attitudes toward the question can be given the semblance of scientific meaning.

1. *Nonsense.* The whole question resembles a tale told by an idiot, full of sound and fury, signifying nothing. The problem may belong in metaphysics, but it has no place in science. Many scientists would accept such a view of the case, and would emphatically deny that they have any metaphysical preconceptions whatever in their work. This would be especially true among experimental and applied scientists. In theoretical science, however, there is an increasing tolerance today for discussion of basic philosophical assumptions underlying scientific work.

2. *Neutral Monism.* It is impossible to specify the nature of events which enter into scientific concepts. They are what they are is all that can be said about them. It is possible to specify the conditions under which they are observed and to use the events in testing the validity of concepts; but with respect to assignment to basic categories they are neutral, neither mental nor physical. In everyday life, to be sure, cer-

tain events are *interpreted* as physical, others as mental; but the psychological increment involved in such attitudes can not be used retroactively to characterize the events themselves. This view is undoubtedly the most sensible, as well as the safest, of the four.

3. *Physical Monism.* As scientific interest spread out from astronomy into the various branches of physics and chemistry, and later into the fields of biology and psychology, the same methodological principles were adapted to the new problems. It is an easy step from a methodology to a metaphysics. That is to say, the materials which show themselves amenable to the *methods* of physics come to be regarded for that reason as inherently 'physical.' The faith that the whole world of experience will eventually yield to the methods of physics may be called a physical monism. Taken strictly, however, such a view is a methodological, not a metaphysical monism. It is so frequently taken to be the latter, however, that the fine point of difference, since it probably rarely exists in the minds of those who are sympathetic to the view, may be overlooked in order to give a name to a *Weltanschauung* to which large numbers of scientists in widely different fields would subscribe.

4. *Psychical Monism.* Since there can be no knowledge without someone to know it, it is obviously impossible to know the nature of the world apart from those characteristics which may be conferred upon it by the knower. And since human beings will never rid themselves of the conviction that knowledge comes through the mind, it can be argued that all items of knowledge must be basically mental. At such a level, the argument is difficult to refute. Not many scientists, however, unless they have been contaminated with

philosophy, are familiar with it or are inclined to bother with it. In the history of philosophy it has had many professed adherents, but since the view leads so relentlessly to solipsism, it has produced few, if any, sane philosophers who acted as though they really believed in it. At a more critical level the view can have no meaning until it can be shown what is meant by saying that knowledge comes through the mind. It may be that the mind is not mental, in which case the circularity of the statement that all known objects must be mental would be broken. But until the circle can be broken, psychical monism will remain a defensible view of the world.

COMMON ORIGINS

The upshot of the whole matter is drastic, for it involves a radical revision of attitude toward the nature of science in general and of psychology in particular. No science is capable of definition in terms of subject-matter, for the subject-matter of all sciences is the same. Some person, some scientist, makes a report on something which he has observed. It may be the downward movement of a stone, a flash of light, the change of color of a liquid, the crossing of a line by a needle-point, the way in which an animal responds to various lights or sounds or pressures, the effect on a given area of color of changes in the surrounding colors, the length of time required to learn a list of words and the number of words which can be recalled after a certain lapse of time, the manner in which people are influenced by newspaper headlines or by pronouncements of those in authority, the speed with which a problem can be solved: the list of possible observations can obviously be run on indefinitely. Some of them are accurate, others turn out to be questionable. Some of them

can be made directly and quite simply. Others require much skill and the aid of elaborate apparatus. The objects reported in the observations differ among themselves in quality, intensity, duration, size, shape, location, susceptibility to change by neighboring objects, attractiveness, usefulness, and numerous other dimensions. Some dimensions are relatively easy to deal with, as, for example, differences in size or intensity. Others, such as the effect on business of a change in political party, are elusive and intricate, and are fraught with the danger of inaccurate report. One of the first tasks of science is to eliminate, as best it can, the sources of error involved in observing objects in complicated relationships. The constant effort on the part of scientists to work with the simpler dimensions, whenever it is feasible, comes from the laudable desire for accuracy, not from any unwillingness to deal with intricacies.

Whenever two or more objects differ from one another with respect to some consistently reportable characteristic, the difference in question can often be erected into a dimension in relation to which a whole class of objects may then be described. Two objects differ in size. Size is then abstracted into a dimension which serves to describe a vast number of different objects. Other dimensions are developed in the same way, and in many cases the same object may be described by reference to several different dimensions. Certain dimensions, moreover, are capable of quantitative treatment, *i.e.,* they extend uniformly between maximum extremes or opposites. It is therefore possible to describe some objects by reference to definite points along such dimensions and to assign to them, on the basis of their relative positions, numerical values of one sort or another. Length, weight, and

time are such dimensions, and physics very early perfected the *cgs* system for the effective manipulation of such dimensions and their derivatives.

It is clearly the ideal of all sciences to force their dimensions into quantitative expression. "*J'affirme,*" said Delboeuf, "*que tant qu'un phénomène, quel qu'il soit, physique ou moral, n'a pas été traduit en nombres, il laisse dans l'esprit toujours quelque chose de mystérieux.*" The older sciences have been eminently successful in this regard, partly because they have limited themselves to those aspects of objects which are most easily measured. The newer sciences have met with greater difficulties and with frequent failures, but also with some remarkable successes. The continuously expanding application of quantitative methods to psychological problems is only one of numerous indications that there is no basic difference in kind between observations in physics and those carried out in psychology.

Descriptive dimensions, no matter in what branch of science they are used, serve merely to bring into relief, and also into systematic coherence, the observable properties possessed by the objects under investigation. They do not permit classification of objects into basic metaphysical categories, such as physical and mental. An astronomer, for example, will observe, often with the aid of elaborate apparatus, the course of a light as registered on a sensitive plate or perhaps as seen more directly in a mirror, and on the basis of many such observations will make inference regarding the source, orbits, and speeds of meteors. A psychologist, or someone instructed to make the observations for him, will note the relative brightnesses of lights presented in pairs, and from the protocols thus obtained will build concepts about the thresh-

olds of consciousness and physiological mechanisms of discrimination. Meteors and consciousness! Can contrasts be greater? Surely such different things do not belong to the same category of existence. Yet the scientific validity of any proposition about either can be tested only by going back to the initial observations, which, in this instance, were both made on visual subject-matter. In other words, the laws about meteors (physical bodies) and those about sensory acuity (mental phenomena) are both constructed out of the same sort of observational stuff. In one case the visual material is described chiefly in terms of a space-time dimension; in the other, the dimension of intensity is used. The same material is analyzed in terms of two different dimensions, but neither dimension furnishes any basis for the differentiation of the material into two separate ontological states. Any separation which occurs at subsequent stages of concept-formation is therefore scientifically meaningless as well as gratuitous, no matter how useful it may be for ordinary purposes of conversation, for it can not be validated by evidence from any of the earlier stages.

The validity of scientific laws is in no way affected by the ontological nature of the objects to which they refer. Description by dimensions creates laws which explain objects-in-relation, and the logic of relationships does not require a knowledge of the nature of the objects related. Similarity of faces can be noted without knowing whether the persons whose faces are thus described are male or female, Mongolian or mulatto. Even less is it necessary to know, as conclusively demonstrated by the fact that so far it has been impossible to discover, whether two objects are mental or physical.

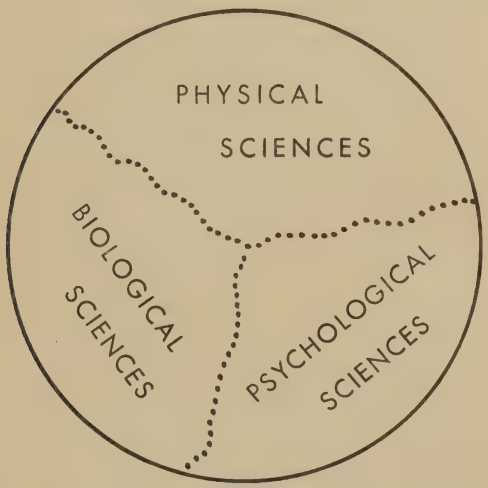
MEANING OF MIND

Psychology is supposed to be, at least etymologically, the study of mind. Within a strict scientific universe of discourse, however, there is no such thing as mind—at least, not with a capital "m." In everyday conversation the word is useful enough, in spite of its gaudy ambiguity of meaning; but in scientific language, except as a short-cut expression, it has no defensible place. Its career would almost be enough to rule it out. In the beginning, mind was inextricably bound up with theological verbiage, but by the middle of last century it had completely lost its soul at the hands of those who were trying to establish a scientific psychology. During the early years of the present century it began to lose consciousness under the blows of behaviorism. Finally, at the present time, even its behavior is questionable. Nay more, its very existence is in doubt. Endowed in youth with a wealth of meaning, it comes to the end of its days in abject poverty. In scientific circles its death will not be mourned; it will go the way of countless other concepts too lacking in vitality to carry on. Outside of scientific circles, Mind will never die. It will live on, a verbal ghost, useful in conversation and pleasant to the ear; but for all scientific intents and purposes the concept has already outlived its usefulness.

DIVISIONS OF SCIENCE

Within the circle on the next page are included all of the initial data of observation which can be put to scientific use. Ideally the size of the circle may be regarded as fixed by the number of properties in the world which can be treated by the methods of science. Actually, however, the size is always

changing. New methods, and especially new apparatus, like the telescope and microscope, open up wide fields of observational material which formerly were completely inaccessible; and critical examination of scientific concepts is constantly revealing, and therefore rejecting, spurious data, data incorrectly reported, and data which are private in the sense that they may be correct enough for the person who thinks he has



them but can not be verified by others. Experiences which can be had only by one person are, for that very reason, beyond the pale. Science is a matter of social verification and acceptance, not an affair of private conviction.

As science gradually emerged from its theological beginnings, it regarded the world with an eye for those events or data which were palpable and also amenable to arrangement in some sort of order. From simple uniformities among such data with respect to dimensions of space, mass, and time, generalizations were made into laws which were believed to

hold for a much larger range of similar data. The laws of motion were not based upon an examination of the speed of movement of objects of every conceivable size and mass. A few well chosen examples were enough for mathematics to go ahead and finish the job. Or rather, in such cases, the job is called finished and the laws held to be correct until contrary evidence is turned up. If the contrary evidence is validated, the laws are revised—which does not necessarily mean that Nature is inconstant. The question as to whether Nature, apart from the glimpses which science catches of her, is *really* and *truly* fickle or not can only be argued by metaphysicians. A scientific law can obviously be no better than the human ingenuity of those who assist in formulating it. The physical sciences have developed a set of laws, for many of which the probable errors have remained practically negligible for a considerable period of time, as human time goes. The faith that laws with small probable errors will sometime be formulated for all the data within the circumference of the circle is the motive-power for nearly all scientific work. Men of science must proceed on the assumption that the objects which fall within their interest and scope of inquiry are subject to law. A scientific law, no matter how complex, rests on the basic proposition, if *a,* then *b.* To proceed on any other assumption is scientific heresy, and deserves the punishment of excommunication. Yet there are men who call themselves scientists and believe at the same time in a principle of indeterminism. The law which will explain such a paradox will one day have to be formulated by psychopathology.

After the physical sciences (astronomy, physics, and chemistry, and their various subdivisions) had mapped out for

themselves a certain sector in the circle of human experience, men arose who insisted that another sector, roughly characterized by the possession of a vague property called *life,* should be studied by the same methods which the older sciences had found so profitable. The struggles of the biological sciences to secure a place for themselves were long and often bitter, and are not yet completely ended. For the most part, however, they have gained full recognition in the family of sciences, as attested by the fact that most philosophers and even theologians are now willing to admit that biologists are justified in trying to formulate their propositions in a terminology which is coming to be more and more indistinguishable from that used by the physical sciences.

The position of the line dividing the biological and physical sectors is variable and in the last analysis quite arbitrary. In the early stages of a new scientific development, observations are at a rather gross level. Animals and plants reveal properties to the unaided eye which demand some sort of preliminary description and classification. At such a level the differences between physical and biological objects seem reasonably clear, and a line separating them can be drawn. But when in the interest of greater accuracy of explanation the observations become more refined, the difference tends to disappear. Speed and extent of movement under specially isolated conditions, repeated application of quantitatively controlled stimuli, examination of electrical transmission in small preparations of tissue, observation of coloration under a microscope—the initial data in such observations resemble closely those which the physicist makes use of. The line separating the physical and biological sciences retains a verbal existence, but in every other respect has vanished. The

data, the methodology, and even the types of theory are all within the same universe of discourse in physics and biology —as illustrated by the names of many of their subdivisions, such as biochemistry, phytogeography, astrophysics, physical chemistry, and the like.

PSYCHOLOGY

The data of the physical and biological sciences do not exhaust the circle of human experience. Language is replete with words, like interest, perception, idea, emotion, memory, discrimination, attention, motive, purpose, *etc.,* which refer to events which seem to fall outside the sectors occupied by the older sciences. They have even been regarded as belonging to an entirely different category of existence. If any line of separation can be drawn at all, it is that between mental events and those which belong to external (physical and biological) bodies. It was with such an apparently clean-cut distinction that psychology began its scientific career. We have already seen, however, with what difficulties psychology was soon beset in its attempts at definition in terms of unique subject-matter.

Since psychology is a relatively young discipline, the preliminary problem of mere classification of its materials is still far from satisfactory solution. In fact, no systematic classification has ever been made. Fechner and Wundt, around 1860, plunged directly into the technique of experimental analysis, whereas zoology and botany were in possession of their *Systema naturae* two hundred years ago, long before the introduction into biology of experimental procedures comparable to those now in use. It might have been better had psychology produced a Linnaeus instead of a Wundt

during its formative years. At all events, the absence of a rigid nomenclature has caused no end of confusion in psychology. The words for objects of psychological investigation are generally taken directly from ordinary speech, and are therefore burdened with ambiguity. There is no need to draw up a list of such words to illustrate the distressing equivocality of the vocabulary with which psychology still struggles. Many controversies in psychology are purely verbal. The adversaries are simply not talking about the same thing. A ruthless insistence upon univocal definition, along lines to be suggested in a later section, may in time remedy this deplorable situation.

At first glance, for reasons implied in the preceding paragraph, psychological objects appear quite different from those in the other sectors. Many of them, since they can still be treated only at a superficial and common-sense level, appear to have only a remote resemblance to so-called physical data. As observations become more exact and refined, however, they differ less and less from those in the older sciences, until in certain fields it is already impossible to say whether a given investigation belongs in physics, physiology, or psychology. Reflexology is both physiology and psychology. Those who study emotions would be hard put to it to say which field they are working in. One of the central problems of psychology, learning and memory, is well over into physiology. And the whole sensory and perceptual field now belongs indifferently to physics, physiology, and psychology. This last circumstance is the crux of the whole matter.

Good scientific observation requires that an object shall be palpable. And palpability, if it has any meaning at all, implies the presence of reportable material within the sensory

fields. All sciences start from the same world of experience. The statement is made so often that it is now almost platitudinous. New developments in science, however, often appear to deal with entirely different kinds of materials; and often enough the descriptive categories remain for a long time at a level difficult to identify with that of the older sciences. Eventually the observations are relentlessly and of scientific necessity narrowed down to palpable items: the data of sense. Sensory material is the Alpha and Omega of science. Within this material there are no ontological differences, but out of this material concepts of every kind and description may be developed. No limit can be set to them, other than that fixed by the limitations of human imagination. However, concepts are only words, and their scientific validity has one and only one test: the logical test of tracing them back to the place where they started. If the test fails, the concept is deprived of good standing in scientific society. Every attempt to trace Mind back to a set of unique materials has failed. The physical sciences, the biological sciences, and the mental sciences all make use of the same kind of observational data. Psychology is a division of scientific labor, not a subject-matter.

CHAPTER II

Objections Overruled

THE CONCLUSION ARRIVED at in the preceding chapter will naturally meet with strong opposition from various quarters. The resistance to it is fortified in many instances by the sort of powerful emotional prejudice against which no amount of rational argument can prevail. The effect of words and of the objects which they seem to intend is often hypnotic. The two verbal worlds, mind and matter, possess many words and phrases whose meanings appear mutually exclusive, and for the great majority of people the distinctions between them remain irrationally fast from childhood on—irrational because such people never give the problem any thought at all, but merely accept without further ado the meanings of words as originally learned. To them the suggestion that mind and matter can not be properly distinguished would seem too fantastic to entertain for a moment. Or if by some strange trick of propaganda the issue could be made to appear vitally important to them and their kind, they would arise in religious wrath and meet their adversaries, not with reasons, but with bullets.

A few souls must indeed exist who under no circumstances whatsoever could be persuaded to take the question seriously. For them it would not matter what mind is, nor would they ever mind what matter is. They would exclaim with Hamlet,

"Words, words, words"—but unlike Hamlet, they would go lightheartedly on their way, rejoicing in their own superior wisdom and sanity. Such gift of detachment and of seeing things in long perspective is fortunate, but unluckily it is not possessed by most mortals. Even psychologists themselves, who ought to be most alert to the equivocal verbiage which clutters their discipline, are prone to take the pronouncements of the profession with unrelieved solemnity.

The view that mind and matter are indistinguishable has many objections, to which some answers are already implied in the arguments used in defense of that position. Further answers will appear in later sections. One or two persistent and common objections, however, should be singled out and disposed of at once. The most obstinate resistance—but fortunately, except for the emotional conflicts aroused, the easiest to meet—comes from common sense and philosophy.

COMMON SENSE

Every person lives in direct and intimate contact with his own self and his own mind. The first-hand knowledge thus gained is accepted at its face value, and any denial of its validity is often met with the scorn reserved for those who parade second-hand information. "You say that I have no mind? Well I know better!—And that's that." The same attitude has been present all through the history of science, for access to the appearance of things in the outside world is no less direct than to the self or mind. The sun still appears to come up out of the east and go down in the west. The moon looks larger at the horizon than at the zenith, and adjacent stars do not look as though they were thousands of light-years apart. No one can *see* the evolution of animal species.

Man is not a cousin, even several times removed, of gorillas and baboons. So it would appear. Yet contrary assertions are now very widely accepted. The older sciences have succeeded in fostering, even among laymen, a willingness to believe that with respect to the outside world things are not what they seem. Only in psychology do people still cling to the right to dismiss as absurd all concepts which do not harmonize with their cherished convictions. Only to psychologists is the philosopher bold enough to argue that when psychology "claims exclusiveness and self-sufficiency, then it behooves the common sense of mankind to remind it of its sources." [9]

What are the sources to which psychology, at the urgent request of an alarmed common sense, must return? It is the self or mind, as everyone knows these things to be. It is the mind as directly revealed to immediate intuition.

> We recur to our positive proposition, that the word "self" must mean to us what we know the self to be. In regard to psychology, more than to any other science, mankind has profound concern in its first principles, and certain inalienable rights in regard to the resulting portrait, which is, after all, its own. It is the more important that these rights be honestly exercised, since there is a deep tendency in human nature to *become like* that which we imagine ourselves to be. It is not a matter of indifference if a human being accepts an account of himself in terms devoid of meaning, of value, of purpose and of conscience.[10]

If common sense had insisted upon its inalienable right to make a portrait of the external world to suit its own intuitions, we should still be hearing a good deal from those whose observations, as far as they go, lead relentlessly to the

[9] W. E. Hocking, *The Self, its Body and Freedom,* 1928, 39.
[10] *Ibid.,* 45.

conclusion that the earth is flat. In the natural sciences intuition has repeatedly been proven wrong. What grounds are there for supposing that in the mental sciences intuition is infallible? Now it should be made clear at once that the sources to which those who place common sense and intuition on a pedestal insist that psychology should return, are, in point of fact, the very sources which psychology, along with every other science, actually does make use of all the time. If by intuition is meant the direct awareness of happenings of one sort or another, then it must be freely admitted, of course, that such intuition is the starting point of all science. But between the events at the starting point and the concepts with which science finally returns to *explain* these same events, there is a long road into a realm of mathematics and logic where in many cases the portraits do not correspond with the original at all.

I have settled down to the task of writing these lectures and have drawn up my chairs to my two tables. Two tables! . . . One of them has been familiar to me from earliest years. It is a commonplace object of that environment which I call the world. How shall I describe it? It has extension; it is comparatively permanent; it is coloured; above all it is *substantial*. . . . Table No. 2 is my scientific table. It is a more recent acquaintance and I do not feel so familiar with it. It does not belong to the world previously mentioned—that world which spontaneously appears around me when I open my eyes. . . . My scientific table is mostly emptiness. Sparsely scattered in that emptiness are numerous electric charges rushing about with great speed; but their combined bulk amounts to less than a billionth of the bulk of the table itself. . . . There is nothing *substantial* about my second table. It is nearly all empty space—space pervaded, it is true, by fields of force, but these are assigned to the category of 'influences', not of 'things'. . . . I need not tell you that modern physics

has by delicate test and remorseless logic assured me that my second table is the only one which is really there—wherever 'there' may be. On the other hand I need not tell you that modern physics will never succeed in exorcising that first table. . . . We must bid good-bye to it for the present for we are about to turn from the familiar world to the world revealed by physics.[11]

TWO PORTRAITS OF THE SELF

The two portraits which Eddington gives of the same table are strikingly different, and yet the general type of conceptualization into which the scientific table is fitted does not meet, in this day and age, with violent protest. Let a psychologist, however, construct a picture of the self which differs markedly from that presented to intuition, immediately whole books will be written pointing out the errors of his ways and the superiority of the portrait of the self which each person carries about with him. The situation is comparable in every respect to the emotional opposition which new developments in science have always had to face. It is the business of science to examine every naïve intuition, and in the inevitable process of correction certain cherished beliefs have to be given up. Two additional features of scientific method, however, complicate the situation.

Besides correcting initial observations, science seeks to build concepts designed to *explain* the events reported in the observations. The explanatory concepts and the events themselves are not, of course, the same.[12] Hence no surprise should be caused by the discovery that the scientific portrait differs

[11] A. S. Eddington, *The Nature of the Physical World,* 1929, ix-xii.

[12] The apparent discrepancy between this statement and the later account of the meaning of explanation will be resolved in the proper place, p. 147 f.

from the original. The scientific one is couched in terms of the *conditions* responsible for the appearance of the event, and since the conditions are not the same as the event, the scientific portrait must of necessity be different from the original. The apparent proximity of the stars is partly due to their remote distance in the sagittal plane, but *measured* distance in this dimension is not the same thing as *apparent* distance in the horizontal plane, although the latter is partly determined by the former. One of the conditions for the seeing of red is an electrochemical disturbance in the optic fibers produced by electromagnetic undulations of radiant energy falling on the retina about 42,500,000,000,000 times a second. Red is therefore partly explained by this circumstance, but there are those who reject all such explanations because the physical undulations and the correlative physiological changes are not themselves red. Those who thus erroneously interpret the task of scientific method need to be reminded, as Hocking cogently puts it, that "to identify any state of mind with a state of matter in motion is the sort of proposition one can make only when he has renounced the meaning of words and stopped thinking." [13] The quality red quite obviously can not be the same as the conditions which lead up to its appearance, so that to identify red, or any other aspect of the self, with a state of matter in motion is indeed the sort of proposition one can make only when he has stopped thinking. Yet Hocking looks with dark disfavor on modern psychology because its portrait of the self does not coincide with the original.

The self can not be *identified* with any of its conditions—

[13] Hocking, *op. cit.*, 22.

such as, for example, a lowered synaptic resistance, or whatever the obscure physiological mechanism is which underlies memory. What psychology strives to do, all that it could ever want to do, is to explain the self *in terms of* physiological conditions, among which those responsible for perception and memory would play a significant role. When psychology finally arrives at an adequate account of the self, the portrait will differ as radically from the original as does the physicist's table from the ordinary table of common sense. By that time, however, philosophers and psychologists will presumably understand better the logic of scientific method and will therefore refrain from denouncing explanations on the ground that they do not coincide with the events they are intended to account for.

A second feature of scientific method must not be overlooked. The validity of a concept in science is gauged not by any similarity, or lack of it, which it may reveal to the events it is designed to explain, but rather by the success with which one may retrace the steps leading from the events to the concept. In the more highly developed natural sciences this is simply the test of predictability. In psychology the possibility of accurate prediction is so rare that the test has small value. Theoretically, however, it should always be possible to derive the behavior of concrete objects from the manner, no matter how abstract, in which their laws are formulated. From the formulas of statics and dynamics the engineer must be able to find his way back to bridges and automobiles. Eddington's familiar table is not lost when it is transformed into a strange-sounding scientific generalization. "I need not remind you," he says, "that modern physics will never succeed in

exorcising that first table. . . . We must bid good-bye to it for the present for we are about to turn from the familiar world to the scientific world revealed by physics."

So in psychology, it should not be necessary to tell anyone that the familiar self, or the mind as discovered in the intimacies of intuition, can never be exorcised. It is merely put aside, after intuition has furnished enough material to work on, in favor of the scientific portrait furnished by physiology. If it should turn out to be impossible to get from the portrait back to the original, the portrait would then have to be done over or touched up. But no change would have to be made merely because the resemblance between the two was not good. Scientists are not portrait-painters who try to capture a faithful likeness. They strive after the logical spirit of the thing.

When the common sense of mankind claims possession of the only true portrait of the self, psychologists should use their scientific sense to remind common sense of its sources. The genesis of common sense is intuition. Intuition is sometimes correct, but probably more often wrong. Even if it were always right, the resulting portrait would be of no scientific use. It must be subjected to all possible refinement of controlled analysis in order that another portrait may be made which will explain the characteristics revealed in the original. The awareness of red, for example, has always been an item in the original portrait and will forever remain there, but such immediate and incontestably correct intuition would never by itself discover anything about wave-lengths of radiant energy or disturbances along optic fibers. One characteristic of the self is a feeling of continuity with the past, but such a feeling does not deliver up to its possessor any knowl-

edge of the neural conditions responsible for it. When psychology and physiology, by whatever methods they can seize upon, gain some insight into these conditions, the account will not sound like a description of memory, for the good and sufficient reason that it will not be memory, but the *conditions* of memory, that will be involved.

Neither in the intuitions of common sense nor in the concepts based upon more careful observation has critical psychology been able to find the slightest hint of a criterion which would make it possible to show that the data made use of in psychology differ in kind from those used by other sciences. In strict scientific language there is therefore no place for terminology which implies that psychology deals with unique mental material. The protests of an enraged common sense are nothing more than emotional prejudices which have become firmly fixed in linguistic habit. The previous chapter showed what havoc such verbal custom has wrought. Once the source of the confusion is understood and eradicated, the same manner of speech can still be used. The only difference will be that the translation of the words will lead to less ambiguous meanings. As symbols on the printed page, or as sounds, they will remain unaltered. Throughout these pages no effort will be made to avoid the use of numerous words and phrases which, understood in their everyday meaning, imply the existence of unique mental data. It would be altogether too clumsy and circumlocutious to try to create a special vocabulary. Even if it were feasible, the new expressions would soon take on unexpected and undesirable meanings. Constant vigilance, rather than a revolution, is the best way to obtain exact meanings in scientific discussions.

Nothing whatever is lost, except futile controversy, by rec-

ognition and acceptance of the fact that psychology does not possess a subject-matter of its own. The same materials are still there, no matter what verbal, logical, or scientific disposition is made of them. "That which we call a rose by any other name would smell as sweet."

TRANSCENDENTALISM

Another objection to what seem like the ravages of a scientific psychology comes at times, strangely enough, from psychologists themselves, although it is more frequently voiced by philosophers. The similarity, both in method and subject-matter, between so-called mental and physical sciences they freely admit, except for one important point. Psychological events, although resembling at one level of observation those which the natural sciences use, have upon them a reference beyond themselves. They possess meaning, value, and purpose. Meanings involve more than sheer data, values move beyond the thing valued into a nonphysical realm, purposes derive their existence from something which does not yet exist, namely, the future. These characteristics can not be dealt with by science, so it is argued, for they are transcendent, and transcendent phenomena have no abiding place in an objective here-and-now psychology.

The simplest answer to this objection would be a dogmatic denial of the truth of the assertions, were it not that the arguments are evidently made in all sincerity of belief. The study of meaning and purpose has been feverishly pursued by psychology from the very beginning. Nothing in all this work has given the slightest support to the contention that the phenomena in question transcend the limitations of scientific observation. Description of meanings is simple enough. It is

their explanation that creates trouble. The question, however, is not one of adequacy of theory, but whether meanings can be handled by psychology.

Meanings involve a pointing relation. One thing stands for another, as a vertical cross for addition, three sharps for the key of A, or the needle on a speedometer for miles per hour. Descriptions of meaning almost universally report the presence of two or more items standing in a special relationship to each other, like percept to image, image to image, same sensory core to different images, different stimuli to same response, same stimulus to different responses, *etc.* Now the study of items either in constant or in variable relationship to one another is certainly nothing new in science, nor is it outside the competence of scientific method. It would be hard to find any scientific problem which does not involve the study of items in relation. Meaning is therefore no exception. Titchener's famous context theory of meaning is a simple formulation in associationistic terminology of the descriptive facts of core-context (=meaning) relations. That the postulated neural connections do not themselves share the meanings which they are intended to explain is no reproach to the theory. Neither do the optic fibers contain red.

Variability of meaning-relations is a common occurrence. The same percept may have several references, the same stimulus, different responses, and *vice versa.* The problem is indeed a complicated one, but it is not for that reason outside the realm of science. And yet it was for just this reason that McDougall, a number of years ago, came to the conclusion that meanings could not be handled by scientific psychology. He cited, as an illustration of the variability of meaning, the startling difference in effect produced by two telegrams dif-

fering only in one letter: "Your son is dead" as contrasted with "Our son is dead." [14] The difference in effect is obviously not correlated directly with the mere presence or absence of the letter *Y*, but it is equally obvious that this letter is the initial determinant in a series of changes in the nervous system which, if there were a technique for getting at them, would surely reveal the immediate correlate of the dramatic behavior. Similar instances of variability in meaning had already been described abundantly by Külpe and his students at Würzburg, and more recent studies in animal behavior have uncovered comparable phenomena. The failure to find point-to-point correlations between end terms in a long series of interlocking processes is no proof that meaning is unlawful. Only if all the intervening processes were known and it were still impossible to discover uniform connections would it be permissible to entertain McDougall's notion that a scientific account of meaning is a *reductio ad absurdum.*

Certain arguments, or rather comments, offered in favor of mental transcendence are exceedingly bizarre. Meanings and purposes, by virtue of their independence of time and space, are beyond the reach of scientific method. Without the aid of space-time dimensions, science would be well-nigh helpless. These two statements appear to make some sort of syllogistic conclusion to the effect that psychology is therefore incapable of doing anything with meaning and purpose. The major premise needs a bit of critical scrutiny.

Transcendence in this context seems to mean that mind is able to encompass or comprehend a 'physical' object the distance or size of which is incommensurable with the dimensions of thought, or that a memory or a purpose includes a

[14] W. McDougall, *Body and Mind,* 1911, 268.

'physical' past which has gone out of existence or a future yet to be. Such statements are absurd. Thought, memories, and purposes are known by way of *present* content or modes of behavior. These latter, in turn, are generally reported as patterns of sensory data: a visual image, swiftly-moving kinaesthetic-verbal processes, movement of an animal in a certain direction, and the like. The patterns have acquired meanings, so that the visual image, for example, means Eiffel Tower; but the image is not 1000 feet high nor 3000 miles away, any more than a map of the United States is 3000 miles wide. The image has its own size and distance, just as does a map, and recent methods have enabled, in some instances, extremely accurate measurement.[15] Here is no transcendental flight, for the meaningful image does not escape to another object, but is rigidly confined to its own small spatial dimensions. Within these smaller dimensions, moreover, the parts ordinarily retain the same relative positions as in the real object. And yet we can read that

> The body is spatial, whether for the old physics or for the new; it has relations of distance to objects outside of it; its parts are near and far, above or below each other. This is not true of the mind, or of any mental event. . . . We must persistently remind ourselves that the thought of a distant object is not a distant thought, nor the thought of a twisted object a twisted thought.[16]

With respect to memory and purpose, the situation is exactly the same. Images may *mean* the past, but they can not *be* the past. Analysis of behavior may exhibit the influence of

[15] See, for example, P. Busse, Ueber die Gedächtnisstufen und ihre Beziehung zum Aufbau der Wahrnehmungswelt, *Zeitschrift für Psychologie,* 1920, 84, 1-67.

[16] Hocking, *op. cit.,* 28 f.

changes in an organism which occurred at an earlier date, so that in this sense present activity may *reflect* past performance. One can think of the future only during the present. Neither the activity of thought nor its contents are in any real future. An organism may be directed toward a goal which, as object of response, is not yet on the scene, but the movements are determined by conditions which are in the organism at the time the action takes place. It is inexcusable play on words to talk about any real future having an influence on the present. Yet it can be found stated that

> There is probably some peculiar mark in the brain which distinguishes a memory, dated backward, from an anticipation of the same thing, dated forward, and from a very faint perception of the same thing, dated now. But this mark remains a present mark. It is a sign of pastness; but it is not pastness. *The translation of such marks into true time-distance remains the prerogative of the mind; for the mind alone . . . has a grasp of the past in its own nature along with the present and the future.*[17]

The use of meanings in support of transcendence is strangely puzzling, for meanings weaken rather than strengthen the case. The pressing of a wireless key is more truly transcendent than any sensory item involved in meaning, for its effect leaps out into real space and invades regions geographically remote from the point of origin, whereas meanings are rigidly confined to the place and the time of their occurrence. The one exception to this rule would be telepathy, but evidence in this direction is still too dubious to be cited seriously in the present context. Meanings *refer* beyond themselves both in space and in time, but the sensory patterns out of which meanings are constructed

[17] *Ibid.*, 33 f. Italics mine.

remain where they are. We must persistently remind ourselves that the *thought* of a distant object is not a distant object, that the *thought* of a twisted object is not a twisted object, and that the mind has no grasp of the past and future, only memories and anticipations. If transcendentalism has any meaning at all, it can be demonstrated in the physical rather than the psychological branches of science. A beam of light from Betelgeuse reaches a given point countless miles away, many years later.

Transcendental arguments have been examined so carefully and answered so devastatingly by Lashley, that the interested reader is referred to this author if he wishes to pursue the topic further.

> There is an almost universal demand that psychology shall do more than explain mind in the sense in which other sciences explain their material. It must also subject itself to anthropocentric values; it must leave room for human ideals and aspirations; and it must present its material in such a way as to identify the explanatory principles with some qualitative elements within the reader's experience.
>
> Other sciences have escaped from this thralldom. The astronomer and biologist no longer need to bow before man's egotism, and their conclusions are a frank denial of his preeminence. And equally they are freed from the necessity of arousing the 'experience of the thing described.' No one asks that the physicist's account of gravity shall make his hearer feel heavier, or that the biologist shall throw him again *in utero* by his statement of the recapitulation theory.
>
> Yet many psychologists demand that the explanation of mind shall be, somehow or other, identical with mind. The final objection to behaviorism is that it just fails to express the vital, personal quality of experience.[18]

[18] K. S. Lashley, Behaviorism and consciousness, *Psychological Review*, 1923, 30, 346.

Lashley's brilliant attack upon transcendentalism was inspired by the radical behaviorism of the last decade, and it was against this barrage that certain philosophical strategists, aided by a few psychologists, directed their counter-attack. Behaviorism deserves lasting credit for pointing out the presence of many useless and unjustifiable concepts within the systematic writings of classical psychology; but in its zeal to purge and reform, behaviorism occasionally fell victim to the hallucinations of an overheated imagination. It saw evil where no evil was. Classical psychology, except in the hands of isolated writers, was no more transcendental than is behaviorism itself. In 1911, before behaviorism had become fully articulate, McDougall was lamenting the steady movement away from transcendentalism which had been going on in psychology for some years.

INTROSPECTION

It is nevertheless true that in classical psychology the view was often expressed that mental events are *subjective* and only obtainable by a unique method of observation called *introspection.* Since behaviorism set itself up as an *objective* discipline, it became almost a point of honor to attack every psychological concept which was suspected of concealing the blemish of the subjective, and to throw into the discard the method which had led to the contamination. The difficulties of giving an adequate definition of the words 'objective' and 'subjective' will appear in a more appropriate place later.[19] In the present context it is important only to point out that most behaviorists committed the logical fallacy of supposing that since nearly all transcendentalists subscribe to a sub-

[19] See p. 93 f.

jective view of mind (however the phrase be defined), and since classical psychologists described consciousness as subjective (whatever that phrase meant), therefore all classical psychologists were transcendentalists. Armed with such logic, they then proceeded to attack classical psychology not only for a transcendentalism which it did not profess, but also for its use of introspection—a method which, if examined operationally, appears to differ in no essential detail from the observational procedure used by all sciences, not excluding behaviorism.

Lashley would relegate introspection "to a subordinate place as an example of the pathology of scientific method." [20] If introspection is pathological, then so is all scientific observation. The word introspection, to be sure, is unfortunate, for it carries with it the implication of a mind turning in upon itself to examine its own internal, hence subjective, workings, and in ordinary speech it often means a morbid analysis of one's own motives of conduct. It should be remembered, however, in extenuation of classical psychology, that the construction of psychological concepts during the middle of the last century was hampered by the domineering influence of a strong philosophical parent who begrudged every show of independence by his wayward child. The child, in turn, could not, even with the best will in the world, come wholly clean of such influence. And in its more mature years psychology can not help the misunderstanding of its concepts brought about by the use of the same or similar words in popular speech.

In practice, introspection involves the report, preferably by a person trained in the procedure, under conditions which

[20] *Op. cit.*, 338.

can be controlled and reproduced, of some happening which takes place in a sector of his experience which he has been set to watch. If it is color that is being investigated, there is no difference in the procedure for reporting it from that used in reporting the movement of a falling body or the course followed by a rat in running a maze. Neither the rat nor a red can report itself. Someone must be on watch to see what happens. Someone must make an observation. Someone must introspect. If red is said to be an unfair example because it is too simple and 'objective,' let any other so-called introspective item be chosen. Images are commonly regarded as peculiarly subjective, the reason being, apparently, that the conditions for their arousal are not external to the person reporting them. Neither are the most important conditions for red external to the observer, but that does not prevent red from coming within the range of report. The geographical location of the conditions of an event does not make or break the report. The lunar cause of tidal movement is far more remote than the neural cause of images. One would have difficulty in finding any connection between the locus of causes and the reportability of their effects.

It is quite true that one of the conditions of perceived red, namely, the dimensions of the external stimulus, is more easily brought under control than are the conditions for images. Yet some of the latter can be handled effectively enough to produce consistent reports, such as, for example, in the case of after-images, the dimensions and time of exposure of the stimulating object, the size, distance, and illumination of the projection screen, and the moment at which the observer is asked to report what appears in the field surrounding a given fixation-point on the screen. If it were not so, the precise

equations in Emmert's laws of negative after-images could not have been worked out. The conditions for memory images are more elusive, but even so, Ebbinghaus was ingenious enough to manipulate them in a manner to produce results which compare favorably with any subsequent observations made on mnemonic performances of animals.

Whether an image is 'really' there or not when it is reported can fortunately be dismissed as an irrelevant question. Consistency and reproducibility of report are the chief criteria of good scientific observations. In the last analysis it can never be known whether the report of red coincides with a real red, or whether any statement about a rat is sufficient evidence for the existence of a real rat, or whether, for that matter, any of the observations of physicists or astronomers attest 'real presence.' If behaviorism had not been so proud of its epistemological innocence, it might not have tripped and fallen over a solipsism which, although it lies in the path of all science, was side-stepped by the natural scientists without any trouble at all.

In and of themselves, the data reported in scientific observations are of secondary importance. It is only when something is done with them that they achieve significance. Only when they permit of inferences about explanatory mechanisms do they have any value for theoretical science. Once the mechanisms have been decently established, the initial data disappear from the record. It must not be forgotten, however, that a record of the data had to be made before the mechanisms of explanation could be substantiated. Data respecting images and perceptions, which the extreme behaviorist does not admit into his science because of their involvement with introspection, will eventually disappear

from psychology, but only *after* physiology has found a way to explain them.

I am exceedingly astigmatic. To my uncorrected vision the moon appears as seven dim and overlapping moons. Now I might construct an account of the world in terms of my astigmatism. It would differ in many ways from an account written by a normal man. It would be true and real for me, but it would omit many details observed by the normal man and would add nothing to his account which he could not predict from the optical principles underlying astigmatism. To the normal man it would be of interest only as an account of the effects of astigmatism. And as soon as I obtain adequate correction, my former account becomes for me also only a pathology of the eye.

The parallel holds for introspection and behaviorism. The subjective view is a partial and distorted analysis. Behaviorism presents the possibility of a more nearly complete analysis of the same data.[21]

It is difficult to know what to make of such a statement. In the first place, no science ever tries to secure a complete account of a given group of phenomena. It merely samples and interpolates. An oculist can judge the extent of astigmatism from reports on the relative clearness of two lines crossing at right angles. To construct *an account of the world* in terms of astigmatism would be wholly unnecessary. Secondly, it is questionable whether asymmetry of curvature of the meridians of cornea and lens would be known as the underlying *principle* of astigmatism if the gross *fact* of astigmatism (*i.e.,* the discovery that a few people see objects differently from the way the majority of people do) were not already familiar. In point of time, at any rate, facts precede their explanation. Instances of visual distortions were known long before any information was to be had about the anatomy

[21] Lashley, *op. cit.,* 337 f.

of the eye. With the growth of knowledge about ocular meridians, correlations between their degrees of curvature and various visual processes, including those of astigmatism, became possible. A correlation which has one of its terms missing (*i.e.,* asymmetry of curvature with no knowledge of visual distortion) would not be a principle. Asymmetry of curvature by itself is merely a fact. It becomes a principle only when it is found to have some relation to other facts, such as those of astigmatism.

In view, thirdly, of his distrust of introspective reports, the behaviorist prefers an objective method as offering "the possibility of a more nearly complete analysis of the same data." In the case of astigmatism, the job could be done 'objectively,' but it would require a lot of patience and a good deal of ingenuity. No words, either spoken or written, could ever pass between experimenter and subject. By applying electric shocks, the experimenter could determine the extent to which his subject was able to respond differentially to various forms, including a large circle like a moon. Among a number of people examined in this fashion, one might be found whose responses differed markedly from those of the other subjects. The experimenter, suspecting some ocular defect, would then proceed to try out various corrective glasses on this fascinating individual. Finally he might succeed in deducing pretty accurately the degree of asymmetry in the curvatures of this particular person's ocular meridians. But why go to such time and trouble? Would a physicist ever take photographs of the eye-movements of another physicist in order to have objective corroborative evidence of the latter's statement that he was counting the swings of a pendulum? "To my uncorrected vision the moon appears as seven dim

and overlapping moons" is an unimpeachable scientific protocol, unless made by a person under the influence of alcohol. Even then, the statement would have its value. The pink elephants of epistemological fame might under certain circumstances have immense psychological value, in spite of, or rather, because of the infrequency with which they are reported by those who return from the circus.

The statement that "the subjective view is a partial and distorted analysis," even if it were true, would not deprive such a view of its importance as a starting point for psychological inquiry. If by subjective view is meant one that depends for its existence upon the person possessing it, then all science is subjective. Objective knowledge, in this sense, can mean nothing more than the cancellation of idiosyncrasies by the process of pooling, sifting, and weighing all the bits of individual subjectivities. The result is still subjective, for there is none other available, unless one likes to believe in a sort of group-science (comparable to the Group Mind) which, detached from its sources, rises to platonic heights of untouchable objectivity.

Whenever, in a scientific setting, a given protocol is returned with reproducible consistency, it becomes the starting point for the formation of propositions and concepts. If the same object of inquiry is subsequently observed from a number of different angles and by the aid of new technical devices, the protocols will be different; but the propositions derived from the first protocols do not for that reason lose their validity. The statement that the moon appears larger at the horizon than at the zenith is not incompatible with the statement that the moon is always the same size. The first statement is not a distortion. It is a fact, just as much as

astigmatism is. To throw it away with the comment that it is mere visual pathology would be wholly unjustifiable. An illusion, if the apparent change in size of the moon may be called such, needs explanation fully as much as any other datum that falls to the lot of psychology to deal with. At present there is no explanation. When one is found, physiology will come into possession of another important principle.

Normal and abnormal are terms of convenience. Scientifically there is nothing either normal or abnormal, but thinking makes it so. The events which are thus labeled extend along an unbroken continuum, no one part of which can be omitted without leaving a gap in scientific theory. "As soon as I obtain adequate correction (for effects of astigmatism)," says Lashley, "my former account becomes for me also only a pathology of the eye." It is a fortunate thing for mankind that medical science runs no danger of being argued out of existence on the grounds that disease, when corrected, turns out to be nothing but a pathology of the body.

The practice of introspection will never disappear from psychology. It might help matters if the word would vanish, as indeed it is already tending to do, but the procedure will continue as long as science itself does. Somewhere within every scientific protocol there must be an introspective datum —*i.e.,* a statement which someone has made about an occurrence which he has observed. It is platitudinous, of course, to remark that efforts are constantly being made to improve methods of observation. Introspection is no exception. Behaviorism did first-rate service in keeping it from being an exception, for its early blasts gave classical psychology a jolt, especially in connection with the sloppy habits of introspection into which it had fallen. Introspections range all the way

from unrestrained fantasies carried on in the comfort and privacy of an armchair to the restricted reports called for in psychophysics where the experimental conditions are fixed as carefully as possible. The latter type of observation is plainly of much greater scientific value than the former.

Identification of introspection with general scientific observation follows from simple epistemological considerations as well as from the thesis outlined in the previous chapter. There are only two sources of scientific knowledge: empirical data, and the manipulation of data by the instruments of logic. Since no ontological criteria have been found for dividing data into subjective or objective or into mental or physical compartments, it would appear that throughout their whole range, observation of them must be of the same basic nature. Introspection, therefore, as a word, is a misnomer, since it implies a special technique for the observation of unique materials. Elimination of the word, even if it were possible, would not, however, eliminate the practice. Hence there need be no grief over the death of Mind on the score that along with it would go the only means by which it could be witnessed, in case it were to return. The fashions of introspection, even those which now clothe a skeleton, will ever be ready against the return of departed spirits. Nor need the behaviorist be alarmed or grieved by the survival of introspection, for introspection is merely another word for observation, and is no more transcendental than any practice that behaviorism itself uses to secure reports on initial data.

RULES OF THE GAME

Only the transcendentalist has cause for grief. His defenses are breaking down all around him, and nothing is in sight to

repair them. To the transcendentalist least of all can modern psychology afford to give comfort, for in return psychology receives nothing of any use. In flatly denying the adequacy of scientific descriptions and explanations, transcendentalism displays its barren negativism. It offers no substitute, no rules by which greater adequacy may be achieved. Mystery is made still more mysterious. Science can only deal with the *fact* of causal connection. The *why* remains unanswerable. The objections which transcendentalism raises to the practices and principles of science, such as those which relate to the establishment of uniform connections between observable events, can therefore not be sustained. In decrying modern scientific psychology transcendentalism is really trying to break the rules of science in general.

It is not so much that a transcendental view of Mind is wrong. Certainly there is no infallible proof that it is wrong. Science after all is only one of the games played by the children of this world, and it may very well be that those who prefer other games are in their generation wiser. It must be remembered, however, that *science is a game* and that those who play it have a right to insist that it be played according to rule. If some of the players, or the bystanders, try to change the rules, the game will go to pieces, or at least, it will not be the same game. Science is too serious and well established a game to be entered into lightly or altered, unless it can be shown either that a proposed alteration does not upset the essential features of play, or that the new game is better than the old one. The following chapters deal with some of the rules of the game which are necessary in the conduct of psychological investigations.

CHAPTER III

Logical Empiricism (Operationism)

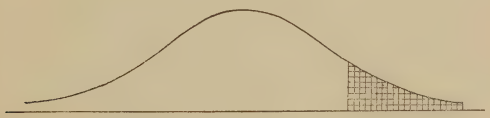
THE AIM OF PHILOSOPHY and of the various special sciences which have evolved out of philosophy is the discovery of truth, the extension of human knowledge. The intrinsic difficulty of the task is often enhanced by numerous complications, certainly not all of which are inextricably bound up with the nature of the objects under investigation. Two of these complications are especially worthy of notice, although the first one will be mentioned only in passing.

PREJUDICE

In 1632 Galileo was commanded by the Inquisition to appear in Rome to answer charges that in his writings he continued to defend two of his propositions which the Congregation of the Index had pronounced invalid, *viz.,* that the sun is immovable in the center of the world, and that the earth has a diurnal motion of rotation. In 1925 a teacher of science in Tennessee was brought to trial and fined one hundred dollars for giving instruction in the Darwinian theory of the origin of species. The path of science is littered with such examples of organized effort to impede its progress. The opposition comes not from reasoned objections, but from prejudice inspired by the fear that any change in the *status quo* will inevitably be for the worse. Victory at one point is

constantly offset by renewed attacks at other points; and since the task of science will never be finished, it is not likely that its future will ever be less troubled than its past.

If the shaded area under the accompanying curve represents the proportion of people in the population who possess a reasonably detached and objective outlook upon life, it is hardly to be supposed that the size of the area will increase appreciably from generation to generation. An enlightened attitude behaves more like a native trait than like an acquired



characteristic. In times of oppression, the size of the area might decrease, for where there is no exposure to truth, even those potentially susceptible will not be infected with the love of it. In times of great enlightenment, the size would probably not exceed the small fixed maximum, for even where there is complete exposure to truth, those who are immune will not be affected. At all times the great majority of the population will therefore remain in a state of ignorance, indifference, incompetence, or positive antagonism in relation to the problem of truth.

At the present time the social sciences suffer most from this state of affairs, for the obvious reason that their views, rather than those of the older sciences, come into most frequent conflict with the tenacious preconceptions of large groups of people. Press, pulpit, radio, entrenched privilege, the power of dead but hallowed form and custom, teachers' oaths, laws, native ignorance and apathy, and the fear of all

of these: the list of agencies which upon occasion may frustrate impartial examination of social issues is a formidable one. In some countries the situation is said to be so serious that anxiety is expressed lest their lamps of learning should go out. One can only hope that it is a passing phase, and that a modicum of tolerance will again return. Even at best, however, science will always find itself, at one point or another, the object of unreasonable attack.

Within science itself prejudice is by no means absent. Men of science are only a little less human than the ordinary run of mankind. Their prejudices are strong, their jealousies often bitter. Since their reward is ordinarily not gold, their vanity must find support in some other way. The substitute is found in all the various devices which make for scientific reputation: degrees, earned and honorary, promotions, earned and unearned, membership in scientific societies and starred citation in various registers, lists of articles and books, frequency of quotation in the writings of other scientists, *etc.* Since within the lifetime of any one individual such reputation, because of the difficulty of getting proper perspective, is fragile, it is often guarded with as much care as the miser gives to his coins. Any suggestion that a given scientific theory is illogical, unsound, or empirically untenable may be passionately resented by its author, and if criticism becomes severe, he may forget his love of truth and remember only his desire for good standing in scientific society. Evidence in favor of the theory is feverishly sought after, evidence against it is minimized or forgotten. Such methods are used in legal debate and political arguments, where truth is of secondary importance. In science they should have no place, but unfortunately they have a large place.

Yet in spite of the unavoidable human frailty of scientists, the work which they collectively accomplish stands as a magnificent monument to the ability of man to arrive at unbiased conclusions. The power of an ideal makes the work of *groups* of scientists more reliable than that done by any individual member. The chain is stronger than the weakest link. The reason for this is simple. The work of scientists is constantly subjected to the fire of criticism. In this fire the errors and prejudices in the work of single individuals are burned away, leaving an intact and purified remainder. The destruction of a theory, or any of its parts, may be mourned by the individuals who developed and supported it, but the group as a whole sheds no tears over the loss. Throughout the world small groups of men, knowing no national boundaries, are inspired by a single high purpose. Throughout the ages their labors contribute slowly to the building of a structure which, for precision and accuracy, is unexcelled by any other human achievement. The only sure knowledge man can ever have will be contained within the structure of science. Other kinds of 'knowledge' may be more useful, more practical as a guide to conduct, more satisfying aesthetically, more comforting in times of need and distress; but none will be as exact as that possessed by science.

PUBLIC KNOWLEDGE

If scientific knowledge could be regarded as identical with the mere having of experience, the task of science would be simple indeed. Every man would be his own scientist, and all of his experiences would immediately become true items in the general body of scientific information. To see a bright flash in the sky and to hear a loud crash a moment later, and

to remember the next day that thunder followed lightning on the preceding day: these experiences, to the person who has them, are indubitable facts. Yet it is plain, after a moment's thought, that they are not facts in quite the same sense in which science speaks of facts. Scientific knowledge is common or public knowledge—or at any rate, it may be had by any intelligent person who will take the trouble to find it out; whereas the facts of immediate experience are private and can be examined only by the person who owns them. Before a fact can be of any use to science, some way must be found to communicate it from one person to another, to change it from private to public property. Observation alone is therefore of no value. It does not become even the first step in scientific method until it is supplemented by words and phrases designed to remove the event observed from the enclosures of unshareable privacy.

Even when language has succeeded in putting a fact on public display, the scientific portion of the public does not immediately accept the fact. A fact is always a function of its method of observation. Exactly where and at what time was it noted? Were any preparations made for the observation, and was the person reporting it prepared to watch carefully? How much training did he have for making such observations? What were the conditions of illumination, temperature, surrounding lights and sounds, proximity of other objects, *etc.?* Was everything possible done to remove all sources of distraction? Was it a difficult observation to make? Could it be repeated easily? If it required auxiliary aids, like special apparatus, precisely how did these operate? How was the observation recorded? Was it written down immediately from memory, or was some other way found of recording it,

as on a smoked drum? Such questions, and many other equally relevant ones, may be asked and must be satisfactorily answered before it can be assumed that a new observation will be received in the community of scientific facts.

The preceding questions all imply an important consideration: no scientific datum can ever be completely independent of the devices used to observe and report it. At some stage in the course of investigation these devices themselves must therefore become the object of careful scrutiny in order that no factor, however unimportant it may appear to be, shall be overlooked which might influence the nature of the datum reported. As the topic of investigation receives wider recognition and is worked over more carefully, the methodological devices are likely to become more or less standardized and hence require less detailed description. Yet the investigator must always be ready to accept any challenge regarding his methods of observation in case there is ever any doubt about his data. The methods, as well as the data themselves, must become public property. All investigators interested in the subject need to know about them. They must not be unique or peculiar to the individual who uses them. The way in which methods are put into common circulation is again by way of linguistic communication. Words and phrases are found which make it possible for one investigator to duplicate the procedures used by another.

From such considerations as these it becomes obvious that even at the simplest level of scientific work the problem of language and the meaning of words assume major importance. Facts, which at first are always private, must be made available to anyone interested in them. They must be described. And since facts, especially the less obvious ones

which require a good deal of effort to get at, are frequently markedly affected by their method of observation, every condition under which they were noted may also require thorough description. Without verbal description and communication, the construction of a body of scientific knowledge would be impossible; and yet, paradoxically enough, the necessity and possibility of using language are at the same time a source of much confusion.

Words stand as symbols between the direct apprehension of facts and the indirect comprehension of them by those who were not present when the events occurred. Even if it were possible for one man to observe all the facts necessary to construct a certain body of knowledge, this circumstance would be of no help. His observations must find some channel of communication to other men, otherwise the knowledge derived from the observations would remain forever private and therefore scientifically non-existent. Symbols for the communication of facts are unavoidable; but it must be remembered that a symbol and a fact are not the same thing. The one stands for the other—or, rather, it should be said that one is *intended* to stand for the other. Unfortunately the intention is not always fully realized. Those who read the symbols may not understand or interpret them in the same way as the person who first used them. Disputes may therefore be due to misinterpretation of symbols rather than to disagreement over facts, although everyone involved in the disputes may be convinced that the question at issue is one of facts, not symbols. Many controversies in science have unquestionably been aggravated by unintentional verbal obscuration. The participants have simply not been talking or thinking about the same thing.

LOGIC OF SCIENCE

In recent years the problem of finding adequate verbal expression for all the various activities involved in science has received serious attention. Never in the history of science has the problem been completely neglected, but it has not often been handled, at least by the scientists themselves, in explicit fashion. As science rapidly climbed to its great heights of success during the last century, it tended to replace philosophy as an instrument for the discovery of truth; and men of science, naturally proud of their many victories, often treated with scorn everything connected with philosophy. The same attitude is common enough even today. How often one may hear it said that science deals with nothing but facts, with nothing but empirical data, in contrast to philosophy, which moves in a nebulous realm of argumentation, in high altitudes of rationalism far above the ground of empirical fact. Yet empirical data constitute only a part of science, and a rather small part at that. The rest is philosophy, or at least, it is that part of philosophy which deals with the laws of reasoning. Logic is an indispensable tool of science, and has been used in the construction of every scientific concept.

For the most part, the logic of science is not a clearly differentiated discipline. It develops out of observational material, and the meaning of its arguments is primarily determined by the sort of relations which exist among the elements of this material. The coercive power of such relations over the course of argumentation prevents the logic of science from going very far wrong, whereas the logic of philosophical dialectics, in view of the frequent impossibility of empirical check, may lead to conclusions which are purely ver-

bal, which have no material meaning. The sequence of words used in the conclusions may be syntactically correct, and yet produce nonsense. They may be as unintelligible as the verses read out by the White Rabbit at the trial of the Knave of Hearts, of which Alice remarked that they had not an atom of meaning, although they appeared to be English.

The logic of science often leaves the ground of empirical fact, but its flights from reality are seldom as lofty as those of metaphysics. One of the rules of the scientific game is that some sort of contact with the ground must always be maintained. An argument without an empirical foundation is no scientific argument at all. Hence the need for careful scrutiny of the processes of logic has not generally impressed itself upon scientists. Their closer contact with reality has given them a feeling of safety. The test of their logic is an empirical one, and if the logic which they use does not check with fact, so much the worse for the logic, whereas in certain types of metaphysical reasoning the opposite attitude seems to prevail. If logic and facts do not agree, so much the worse for the facts.

Scientists constantly make use of logic and mathematics, and the validity, as well as the usefulness, of the applications which they borrow is for them a matter of empirical determination. Logicians and mathematicians, especially those of more strictly formal persuasion, attempt to work out the laws of their disciplines in terms of internal consistency and necessity. Their formulas and verbal structures follow an *ideal* pattern of rational relations. Whether they fit the *real* pattern of empirical configurations is a problem for the scientists to solve. Interchange of ideas has always taken place between logic and science. Of late, however, it has become

more pronounced. Certain logicians have taken it upon themselves to analyze the linguistic processes which intervene between the initial observations of science and the resulting concepts. And scientists in several fields, notably physics, have turned critical attention to logic in an effort to introduce greater precision of meaning into the words used in the formulation of their concepts.

CONCEPTS

The most troublesome problem, so far as the logic of science is concerned, relates to the manner in which concepts are constructed and formulated. To describe observations and methods accurately is difficult enough. To find words to convey the exact meaning of generalities, concepts, laws, principles, and theories is not only still more difficult; it is even dangerous, if not perhaps impossible. It is dangerous, for the words inevitably contain both too much meaning, and too little. Even the most carefully formulated concept transcends the observations on which it is based, and also falls short of them. It lumps together into a word, or phrase, or mathematical symbol, a whole series of observations, made of necessity at different times, under conditions which at best were only *almost* the same. It therefore does not do complete justice to the particular instances which are absorbed in the idealized average statement. But on the other hand, it does more than strict justice by implying, in accordance with scientific convention, greater generality than the particular observations warrant, since all observations must come to an end long before every instance of their occurrence is examined. The extension or meaning of the concept is consequently slightly in error both in the direction of increase and

of decrease. The facts and the concept can not coincide exactly, for at least three reasons.

(1) Facts of observation and the verbal or mathematical concepts for handling them can not be identical, since they are not the same thing. The difference between them may therefore be a source of error or misunderstanding.

(2) Scientific convention, or, what is probably the same thing, the need for verbal simplification, has given to concepts a generality which is at once both greater and less than the facts of observation strictly warrant. An uncertain amount, sometimes negligible, sometimes considerable, depending upon the general level of accuracy of the work being done, would have to be added and subtracted in order to make the edges of the facts coincide unerringly with the edges of the concept.

(3) Even the most meticulous choice of words and symbols can not guarantee their proper understanding on the part of those who read them. Mathematical symbols are the best, the least equivocal. Words may sometimes be the next best. At other times, however, especially if the words have several meanings, they may lead to utter confusion, for they will be understood differently by different readers. Each person may be influenced, perhaps quite unconsciously, by some special association which he has had with the words, and may therefore interpret the concept by an indeterminate increment of meaning peculiar to himself.[22]

[22] A recent study from the Harvard Psychological Clinic bears on this point. Subjects were presented with a list of words. Whenever a certain critical word appeared, they were given an electric shock. After a few repetitions, psychogalvanometric readings showed that a conditioned response had been established for the critical word. The readings also showed, however, that responses had been conditioned to

It must therefore be obvious that if a given field of inquiry is specially complicated, or if great care is not exercised in the choice of symbols, the concepts in use will be deceptive. They will appear to have a meaning, for the words will follow the grammatical pattern of the language from which they are taken; but it may be almost impossible to state precisely *what* meaning they have. For the purposes of science, the concepts may possess nothing but *pseudo*-meanings, and discussions of their proper interpretation may give rise merely to pseudo-problems, verbal problems without material significance.

The logicians who have recently tried most seriously to rid science and philosophy of pseudo-problems call themselves logical positivists, or, as some of their number prefer, logical empiricists. With Carnap as perhaps their best known representative, they have developed a point of view which is extremely radical toward the traditional problems of philosophy. These new empiricists do not claim to be a philosophical school. Indeed, they refuse emphatically to put forward metaphysical pronouncements of any kind. They reject all metaphysical statements, for reasons which follow from their main tenets.

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meaningfully related words, even in cases where the subject was quite unable to recall or think of these words consciously. The neural disturbance occasioned by the critical word apparently subexcited neural conditions leading to words of similar or related meanings. Such results lend physiological confirmation to what has long been known, namely, that some given word, in spite of the best efforts of the writer who uses it, may have an uncontrollable penumbra of meaning which varies from individual to individual. If ten psychologists, for example, were asked to define the word "introvert," the chances of getting complete agreement would be pretty small. K. Diven, Certain determinants in the conditioning of anxiety reactions, *Journal of Psychology,* 1936, 3, 291-308.

Their basic contention is that in order that any proposition should have verifiable and univocal meaning, it must possess verbal relations of implication to statements contained in protocols reporting empirical observations. Scientific propositions always strive to be of such nature, although in many cases they fall short of their ideal. But in proportion as the striving brings success, the propositions acquire more scientific sense, until at the present time there is no field of inquiry open to any decent sort of observation which does not have its share of workers busily engaged in applying scientific methods to it. If logic itself, as an exact discipline dealing with the laws of reasoning, is separated from philosophy, then the only problems left to philosophy are those which can not be treated by scientific methodology. Such problems are pseudo-problems, for any solution to a problem which has no method of solution must be a meaningless solution. Many of the '-isms' of traditional philosophy are just such problems, and logical empiricism will have nothing to do with them. They raise questions to which there are no verifiable answers. The elaborate answers which have been offered throughout the history of philosophy can not be analyzed, by any method yet known, into meaningful statements. The statements are therefore *nonsense*—the word to be given a logical, not an evaluative meaning.

The antagonistic attitude which logical empiricism adopts toward traditional philosophy is undoubtedly for the most part serious and sincere. Yet it is probably true that a certain element of it is intentional exaggeration designed to achieve a pointed effect. No sharp line of division separates the concepts of philosophy from those of science. All concepts have a place somewhere along an unbroken continuum: at one end

are those with the maximum amount of factual support; at the other, those with little or no relation to empirical evidence. It is obvious where the majority of philosophical concepts fall; but it is also obvious that not all scientific concepts enjoy such freedom from logical nonsense and such wealth of observational material that they can boast of a superior status. Otherwise the need for a thorough examination of its own logic would not have been felt by the most exact of all empirical sciences, namely physics.

The revision of many physical concepts required by the work of Einstein has resulted in a searching inquiry by theoretical physicists into the nature of concept-formation in general. This interest has been brought to a focus in the writings of Bridgman, whose method of analysis of the meaning of a scientific concept is known as operationism. Although inspired by somewhat different motives, and interested in different aspects of a many-sided problem, the logicians and operationists share certain convictions in common, and are at one in their attitude toward the definition of the meaning of concepts. The logicians are primarily concerned with the implicative characteristics of concepts in relation to protocol statements, whereas the scientists follow the development of concepts from the starting point of observations. If these two approaches amount to the same thing, they nevertheless make for a difference of emphasis, as seen by the fact that much of the work of logical empiricism at the present time is devoted to what many scientists would regard as an overrefinement of linguistic analysis. This statement is not intended to minimize the brilliance and importance of the logical analyses which Carnap and his colleagues have made of philosophical and scientific ideas. To scientists, however,

the greater concreteness of Bridgman's operationism will probably make a readier appeal.[23]

FORCE

How is it possible to know the meaning of a scientific concept? What does it mean, for example, to say that one body acts upon another body with a certain force? What is *force?* Before it is anything else, it is of course merely a word, and the place to go for the meaning of words is the dictionary. For ordinary purposes the dictionary is valuable, but for the resolution of ambiguous meanings the dictionary is of small assistance, for it lists several meanings, defining them usually in terms of other words (*q.v. ad infinitum*), or by enumerating synonyms (which purists say do not exist) and antonyms (of which there can be only one for a given word, according to the purists, although the dictionary may give several), or by quoting passages from literature in which the words occur. The Shorter Oxford Dictionary lists fifteen meanings of the word "force."

Altogether too much time, in psychology at any rate, is devoted to 'research' of the dictionary sort. Everything which has been written about a given topic constitutes the psychological dictionary of that topic, and anyone working in that particular field is often expected to know its dictionary thoroughly. In view of the fact that the roots of many psychological topics go deep down into ancient philosophy, the task

[23] The literature in both fields is already voluminous. The main trends of thought, and further references, may be found in R. Carnap, Die physikalische Sprache als Universalsprache der Wissenschaft, *Erkenntnis,* 1932, 2, 432-465, revised by the author and translated by M. Black under the title, *The Unity of Science,* 1934, and P. W. Bridgman, *The Logic of Modern Physics,* 1927.

of knowing what all men interested in the topics have said sometimes becomes appalling. It would not be so discouraging if research of that sort were sure to come up with something worth while, but in too many cases the result is *nil,* for the concepts have not been defined operationally, but merely verbally, by analogies, or by reference to the views of other writers. Such historical knowledge can be of importance only to professional historians, and to those people who regard any kind of knowledge important, even when it comes from thinkers who were not quite clear what it was they were thinking about. The history of psychological *experiments* is profitable, but the history of psychological speculation, divorced from experiments, can be weary, stale, and flat.

What, then, is the operational meaning of force? It can only be discovered by finding out just what was *done* and what was *observed,* or, in other words, what *operations* were carried out before the concept came into use. Consider Newton's second law of motion to the effect that force creates momentum in its own direction, and is measured by the rate of change of the momentum created by it. If some object hits a billiard ball resting on the smooth surface of a table, the ball will start to move. It is a matter of direct observation that the speed of movement is some function both of the speed and of the mass of the object which hits the ball. Refinement of observations includes, among other things, standardization of the units of measurement, and control in terms of these units of weights and the rates of movement of the experimental objects. If different forces from a calibrated spring balance pull an object, it then appears that the acceleration of the object is exactly proportional to the force of the pull, so that one may write, $F \propto ma$, or, if absolute units,

such as dynes, are used, $F=ma$. Now if the definition of force is to be given in terms of operations, it must follow that force is nothing more than a word used for characterizing a certain set of relations among objects, observed and measured under controlled conditions by standardized techniques. On the right-hand side of the equation are the symbols which summarize these relations, and, by implication, the operations by which their observation was made possible. Force is *equal to* these relations, and is *nothing but* these same relations. Any notion of a power or a thing or an influence which makes the objects behave as they do is therefore foreign to the scientific concept of force, although the word in ordinary use is full of such meanings.

Undoubtedly the choice of words to designate scientific concepts is dictated, even in a strict discipline like physics, by considerations which have little to do with observations. Otherwise it would be difficult to understand the selection of so many animistic words, like force, power, influence, stress, momentum, inertia, strain, energy, capacity, attraction, repulsion, pressure, resistance, tension, ability, *etc.* Physics can hardly be accused of endowing nature with all sorts of power in order thereby to give a better explanation of natural phenomena; yet many words in physics suggest that the early workers were still under the domination of the very ideology which they were trying to abolish. Or, at all events, they were reading into their observations certain meanings which did not strictly belong there. "In origin the concept (*i.e.,* force) doubtless arises from the muscular sensations of resistance experienced from external bodies. This crude concept may at once be put on a quantitative basis by substituting a spring

balance for our muscles." [24] Physics has been spared many of the crudities which infest psychological meanings, largely as the result of its ability to cast its observations into mathematical form. If the meaning of a concept can be *equated* to a network of relations within which the functional connections have been quantitatively established, it is clearly incorrect to add anything to the meaning which is not included on the other side of the equation.

It is often supposed, and herein lies a grave danger, that the non-operational halo of meaning which surrounds a word contains some unique explanatory power, as when one says that the movement of an object is *due to* a force. The non-operational meaning has no scientific meaning. It therefore must be magic if such a meaning can explain anything. The proper scientific explanation is contained within the formulas themselves. Beyond uniformities of connections no meaningful explanation can go. All scientific explanation is circular. It is perfectly correct, provided one knows what he means, to say that movement of a body is due to a force. But the force is only known by an examination of the movement. To suppose that such a circle can be escaped by endowing force with a meaning other than that derived from the observation of moving objects is to suffer delusion.

ENERGY

Operational analysis of other physical concepts which have a faint animistic tinge to their meaning, like *energy,* reveals much the same state of affairs. The concept of kinetic energy includes, in addition to the meanings already identified with

[24] Bridgman, *op. cit.,* 102.

force, the notion of work. Work is defined as the product of a force and the distance through which the force moves an object during a given unit of time. Work, in this sense, equals half the mass of the object times the square of its velocity, and hence contains no meaning other than that implied by its quantitative definition. The resulting formula for kinetic energy, $KE=\frac{1}{2}mv^2$, therefore introduces not a trace of meaning over and above that implied by the operations which made the formulation of the equation possible. Energy is equal to and is nothing more than a certain set of relations among objects, observed and measured under controlled conditions by standardized techniques. To say that certain happenings are due to energy is permissible, provided one does not mean by energy anything more than the relations which exist among the happenings themselves. Energy is not an agent or a power which does things. It is a concept, a word used merely to describe a set of observations and operations.

If clear recognition of the necessity of operational definitions of concepts is a relatively new insight, how did it happen that physics managed to keep out of the darkness of logical nonsense? The answer that Bridgman would presumably give is that physics has by no means always kept out of the dark. Physicists have been on the search more than once for things which do not exist, like force and energy. Such verbal concepts have been reified, and efforts made to locate them in space and to measure the speed with which they travel. Futile attempts of this kind would not have been made had there been clear realization that force and energy are not *things*. And at certain frontiers of physical research today it is apparently necessary to be extremely cautious in order to reduce the number of "duds" that have recently been

dropping into the field of speculation. Nevertheless it is certainly true that physicists as a group have been taken in by nonsense less often than have the workers in biological and psychological sciences.[25] What is the secret of their protection?

DEFINITION BY EQUATION

Usually there is safety in numbers, provided the probable error is not too high. If the name or the symbol for a concept stands on one side of an equation, and the mathematical symbols which summarize the operations stand on the other side, the danger of a verbal flight from reality is greatly reduced. The correct way to interpret such formulas is to state that the concept is equal to, or proportional to, or some function of the empirical data which are described and restricted by the symbols used in the formulas. The numerical values expressed by the symbols are more firmly fixed than are the values which have to rely on words for their expression, so that if the concept is equal to or some definite function of the numerical values, the reader can not interpret it in accordance with some wayward fancy. The meaning of the concept is held down and fastened by the properties of the equation.

Mathematical formulas, however, can not by any means guarantee complete protection against misunderstanding. The meaning of the formulas must, at some point in the scientific game, be converted into words, and with that necessity comes also the possibility of error, for the words and the ini-

[25] This statement only applies when physicists (and astronomers and chemists) remain within their field of competence. With respect to matters outside their proper field of inquiry, these men give the appearance of being as nonsensical and gullible as anyone else, if not a little more so. It is a pity that transfer of training is not more effective.

tial data can not be identical. The scientific meaning of the words may also change from time to time, thus introducing at least temporary confusion until the new meanings are fixed. Science does not stand still. New observations may require reformulation of concepts. New methods may change the significance of the observations. Nothing is absolute.

Nothing is absolute for a very long time. At any given stage in scientific development, however, the data of observation are absolute for that period, otherwise, if *everything* were relative, there would be *nothing* to test the validity of a concept. A concept can not test itself, nor can it be tested by an item which is itself merely a word. Concepts are words, but the initial data of observation are not words, although they must be converted into words before they can be scientifically useful. The data themselves constitute the final test of validity. Their conversion into words is a constant source of error, but the danger of error can be reduced by describing the operations which made observation of the data possible. If the operations are exactly duplicated, it then usually happens that a second person reports that he sees the same things that the first observer reported. The solipsistic relativity of the meaning of such reports has never bothered physicists, in spite of the fact that it is completely inescapable. It is more than likely that only a very few physicists have ever known that there was such a thing as a solipsistic problem in science. "This position (*i.e.,* solipsism)," says Bridgman, "is often felt to be absurd and contrary to common sense." . . . He goes on to say, however, that it "is a simple statement of what direct observation gives me, and we have got to adjust our thinking so that it will not seem repugnant." [26] Physics

[26] Bridgman, *The Nature of Physical Theory,* 1936, 14 f.

has unconsciously adjusted its thinking to solipsism from the very beginning. It would be a shame if physics were to go neurotic, as psychology has done, with feelings of repugnance and difficulties of adjustment over a problem which, if kept down in the unconscious where it belongs, need cause science no serious embarrassment.

CONCEPTS IN PSYCHOLOGY

The number of concepts in psychology the meanings of which are held down and fastened by the properties of mathematical equations is small, and their significance to psychology as a whole is perhaps even smaller. The famous Fechnerian formula, $S=k \log R$, is not correct. It certainly does not hold at the extremes, and holds only roughly in the middle region. Even when the equation is more accurately formulated, the majority of psychologists today find no occasion whatever to make use of it in any of their investigations. On the other hand, a formula which most psychologists would regard as vastly more important, namely, $IQ=\frac{MA}{CA}$, is rarely regarded as an *equation.*

A number of years ago a group of educational psychologists held a symposium on the topic of intelligence.[27] A perusal of the definitions there offered would lead one to suppose that the occasion was an immoderate return to the literal meaning of symposium. The situation is still much the same. *Intelligence is what the tests test,* and what the tests test is summarized by the value arrived at by dividing the chronological age into the scores which define the mental age. Yet it is possible to find about as many definitions of

[27] Intelligence and its measurement: a symposium, *Journal of Educational Psychology,* 1921, 12, 123-147.

intelligence as there are men who are willing to risk a definition. If *IQ* represents the quantitative expression of intelligence, and if, furthermore, *IQ* is defined by and equated to the operations symbolized on the right-hand side of the equation, as it should be, then the willingness to discuss a host of other definitions must signify either that the meaning of an equation or the logic of concept-formation is not understood, or, these matters being understood, that they are regarded as irrelevant to the problems of psychology.

The literature of psychology is cluttered with concepts which have not been defined operationally. In many instances an operational definition would be impossible, and yet the concepts persist, partly as the result of the prestige which ancient tradition and high authority have conferred upon them, and partly, it may be assumed, because they ring true to the private experience of many psychologists and for that reason are clung to tenaciously, even lovingly.

Extreme operationists would undoubtedly like to wipe out about ninety percent of all psychological concepts, and then start all over again. But to start over again would involve the construction of a whole vocabulary of artificial symbols and the assignment to them of exact meanings, only to discover that after being in circulation for a short time the symbols had acquired quite different meanings. In the present state of psychology the virginity of concepts could not last long enough to create more than a fleeting sense of purity. Decency can only be maintained by a marriage of convenience between ordinary words and operational definitions of them; by the exercise of compromise and tolerance, rather than ruthless ostracism.

CHAPTER IV

Operationism in Psychology

OPERATIONISM IS NO PANACEA for the ills of psychological and social sciences. To err is human, but since scientists are not divine there is small danger that they will overlook and forgive the errors of their colleagues. Elimination of errors is one of the rules of science, and the rule has always been applied vigorously, and always will be—operationism, or no operationism. Nearly the whole burden of scientific methodology concerns the strict application of experimental procedures designed to reduce the possibility of error. All good scientists have been operationists in deed, if not in word. And philosophers like Bacon and J. S. Mill have also been operationists in spirit, if not in deed. Mistakes will nevertheless always be made in scientific work and reasoning. Operationism is the manner in which the present generation utters the familiar cry of science, "Be careful."

It is not always an easy matter, however, to know just how to be careful, or rather, to know where, among the numerous steps which intervene between observation and conclusion, it is necessary to exercise the greatest caution. If operationism can clarify this problem, it will escape the accusation that it is merely saying in new words what science has long since been familiar with.

Scientific methodology may, for the sake of discussion, be divided roughly into three parts.

(1) The use of experimental techniques and the collection of initial data of observation. This part of scientific work is concerned primarily with instruments and apparatus and the use to which they are put, and culminates in the observations themselves and the devices for recording them.

(2) Inference from observations. The relations noted among the data of observation are usually regarded in science as indications of generalities. Laws are stated, hypotheses and theories are proposed, concepts of various kinds are developed. The inductive methods and the whole logic of science and of mathematics are here brought into play.

(3) Deduction of consequences from laws and hypotheses. Since it is ordinarily assumed that laws possess generalities which transcend the particular observations on which they are based, it is regarded as a test of the validity of the laws if the logical consequences which can be deduced from them are verified empirically. The brilliant success which the older sciences have enjoyed in this respect has naturally and quite rightly tended to give them a superior rating in the hierarchy of sciences.

USE OF DEDUCTION IN PSYCHOLOGY

The question may very seriously be raised as to whether psychology is yet in a position to apply the third procedure on more than rather limited scale. In psychology large generalizations which permit of fruitful deductions in various directions do not exist in any sense comparable to the manner in which immediate and verifiable consequences throughout a wide realm of empirical happenings follow, for exam-

ple, from Newton's laws of motion. The law of associationism (or of conditioning), although older and more widely used and discussed than any other psychological principle, has not yet been unequivocally formulated. An impressive array of empirical data furnishes evidence that originally disconnected items in the field of percepts, ideas, and bodily movements come in time to function in relation to one another. But this incontrovertible fact can be observed under such a variety of different conditions that all attempts to extend the fact into a general principle find it necessary either (1) to abstract the fact from its conditions or (2) to enumerate the conditions in a sort of footnote-commentary without knowing the exact number of conditions or their relative importance. Ever since the severe attacks on associationism by the Würzburg psychologists thirty years ago, all attempts at formulating the law have met with strong opposition from various quarters. The opposition is united in its dislike for associationism, but its own diverse formulations are still too vague to permit of rigid deduction of consequences.

An instructive example of the futility of certain kinds of historical research may be drawn from the field of associationism. Aristotle had a good deal to say about association, but its most luxuriant treatment may be found in the writings of the British and Scottish philosophers during the last three hundred years. In these writings the ways in which the laws were formulated are as numerous as were the men who, like dogs worrying an old shoe, pounced on the topic. And what does the student of history get for his trouble? He learns that Hume believed ideas were held together by the force of attraction; that Hartley postulated connections among the vibratiuncles of the brain as the basis for the relation between

repeated sensations and recurring ideas; that Brown drew up three primary and nine secondary laws of association or suggestion; that James Mill reduced these laws to the principles of frequency and vividness, and so on.

Many apparent deductions in psychology are not deductions of *new* consequences in any strict sense at all. They are merely confirmations of what is already known from casual observation. The illusion of deduction comes from the fact that the consequence which is supposedly deduced is already present, but *concealed,* in the hypothesis or major premise. The 'proof' of the mortality of Socrates is not a new fact deduced from the propositions of the premises: all men are mortal, and, Socrates is a man; for the first proposition, namely, *all* men are mortal, can only be true if the man Socrates is already included within its empirical scope.

Those who dislike associationism have argued that frequency of repetition is merely a contingent, not a causal factor in the recognition of familiar objects; that it must therefore be possible under certain conditions to rule out the factor of repetition altogether; and that under such conditions an object which had been made thoroughly familiar would nevertheless fail to be recognized. Experiments have been performed to 'prove' the deduction expressed in the third proposition.[28] Geometrical designs were presented to observers a large number of times. The observers became thoroughly familiar with them and recognized them unfailingly. The designs were presented again, but were now included as parts of larger designs. Under these circumstances the designs not

[28] K. Gottschaldt, Ueber den Einfluss der Erfahrung auf die Wahrnehmung von Figuren, I., *Psychologische Forschung,* 1926, 8, 261-317; II., *ibid.,* 1929, 12, 1-87.

only could not be recognized; in many instances, after deliberate search for them, they could not even be seen, despite the fact that they were staring the observers right in the face. The experiment was shrewdly conceived, and the results were striking, but it can not be maintained that the results validate a new consequence deduced from a large generalization in which the deduction in question was not already empirically contained. They confirm observations which are common enough in everyday life—observations which undoubtedly had a good deal to do with the formulation of the very generalization from which they were supposedly deduced. Is it not a common occurrence for a person to fail to recognize a well-known object in a new environment? [29]

LAW OF ASSOCIATION

These remarks are not intended in the least as a disparagement of Gottschaldt's excellent experiment. They refer only to the difficulty of finding important generalizations in psychology which permit of rigorous and profitable deductions. The deductive process in psychology encounters three major embarrassments, all of which are well illustrated in the law

[29] Recently I went into a museum to look at a picture that had caught my interest on previous visits. It was not in its usual place. I looked all about the room, and in adjacent rooms. Finally I asked an attendant where the picture had disappeared to. He pointed to the opposite wall. I had already looked there, but had completely overlooked the picture because of the changed surroundings.

I once knew an organist in a Catholic church who took mischievous delight in playing Martin Luther's hymn, *Ein' feste Burg,* during improvisations at the close of service. By running the theme as an inner part in close harmony, he was sure no one in the congregation would recognize it. I have heard my father tell of an old choirmaster who once had his chorus sing the doxology very slowly to the tune of Yankee Doodle—and got away with it.

of association. (1) Generalizations tend to run too far beyond the empirical data. The danger of a *non sequitur* in deduction must obviously be proportional to the size of the gap between observation and theory. If the theory sticks close to the facts, speculation in terms of the theory ought not to lead to many consequences which, when tried out experimentally, are discovered to be wrong. If, on the other hand, the theory contains a wild flight of fancy in it, speculation based on it has about as good a chance of being wrong as right. The experimental facts of association come largely from work on memory, but the theory of association is often formulated to include about everything under the psychological sun. When Ach, in his experiments on productive thought, showed that associationism was inadequate to the results, what he really did was merely to demonstrate that a theory which had gone out of bounds was no longer much good.

Even generalizations which stay well within legitimate bounds often run into difficulty in psychology—a difficulty which will be inescapable for a long time to come. (2) If some of the important conditions correlated with the facts upon which a theory is based can not be observed and controlled, it is plain that speculation in terms of the theory may lead to false conclusions.

If a man who knew nothing about baseball were to set himself the task of watching and studying the game as seen on the diamond, it would not take long for him to understand the game perfectly and to anticipate fairly accurately the moves of the players at the crack of the bat. Suppose, however, that his field of vision included only the shortstop and the second baseman and the plays which occurred at second base. After faithful attendance throughout the whole

season, he would find himself at the World's Series still mystified by the behavior of those two men. Think of the number of hypotheses he might make to account for their strange gyrations, only to have them completely upset by some inexplicable deviation, such as a rush of angry men into his sector of vision following a questionable decision by the umpire. So it is with human gyrations in general which the psychologist tries to understand: they reveal certain uniformities and also a wealth of confusing patterns, but even the uniformities are frequently subject to change without notice.

The psychologist is in a position to observe memories, thoughts, and bodily movements, as well as the environment in which these events take place, but some of the most important conditions responsible for the relations which are found to exist among these events are in the nervous system, out of sight, and at the present time, for the most part out of reach. Neither the psychologist nor the physiologist has yet found a point of vantage from which he can watch them. It is therefore not surprising that the data which can be observed are still inadequate to the construction of a logical system which permits of rigorous deductions. Some of the most crucial items are missing. Correlated with every fact of association are certain neural processes the material properties of which are unknown. Every attempt to predict the role which a given set of associations will play in subsequent behavior is precarious business, since no direct knowledge can be had of the changes which take place in the underlying neural processes during a given lapse of time.

To insist that scientific knowledge must be public knowledge is not to deny, of course, that inferences based on private experience may be correct. Their validity can only be known,

however, to the person who makes them. If this person is a psychologist, he may unwittingly weave his own private inferences into his public hypotheses. Or he may include meanings in his hypotheses which come from sets of data different from those upon which the hypotheses are supposedly based, as in the case of generalizations about animal learning, many of which are taken over bodily, although with different terminology, from the work done long ago by Ebbinghaus. In other words, (3) generalizations may conceal correct inferences which come from related, but nevertheless different sources. Empirical verification of the logical consequences which follow from such generalizations really does no more than confirm what is already known in other contexts. They are not predictions in any strict sense. As in the case of reaffirming the mortality of Socrates by syllogistic means, they give the illusion of being true deductions: the empirically established fact was already contained in the generalization itself. There is no harm, of course, in such work, but the person doing it should not delude himself into thinking that he is blazing new trails.

Do these three difficulties make it impossible for psychology to use the deductive procedure? An affirmative answer to this question would be unwarranted.[30] Difficulties with deduction, as they occur in the various sciences, are matters of

[30] C. L. Hull has made the most recent and penetrating effort to apply the deductive procedure to psychological problems. See his article, and its references to some of his other work, Mind, mechanism, and adaptive behavior, *Psychological Review,* 1937, 44, 1-32. The reader need not take too seriously Hull's strictures against classical psychology. They represent the lingering influence of the early behaviorists who apparently believed that scientific logic is applicable only to protocols based on observations of the movements of biological organisms, and to nothing else.

degree, not of kind. All sciences have trouble with deduction. It simply happens that psychology has more than its share. It encounters, indeed, so much trouble, that it is a fair question to ask whether psychology would not profit more from the first two of the three procedures mentioned above, namely, observation and inference, than from a premature concern with deduction.

Deduction on a small scale—that is to say, deduction in terms of hypotheses which remain close to a limited range of careful observations—is another matter. Experiments are rarely made which are not partially dictated by concepts which have grown out of earlier experiments in the same field. Every hunch, every gamble that one type of experiment would be better than another, is in the nature of a deduction. A deduction is still a deduction, even if it is only a guess, even if it is hazily phrased. At the present time numerous small hypotheses in the various fields of psychological inquiry are probably more profitable than any attempt to work out a few large theories intended to cover everything. The latter of necessity have to be too vague to be of real use for purposes of deduction. The operations which would have to be carried out to test such theories can not be stated with sufficient precision, whereas a circumscribed hypothesis, since it is nearer to the operations which dictated it, more readily suggests experiments for its validation. Grandiose theories tend to stir up speculation, but it is not safe to assume that the health of psychology is measured by its wealth of speculation.

COLLECTING DATA (OBSERVATION)

Whenever a complex activity, like science, is discussed from various angles, the differences between these angles, or

parts, tend to be unduly magnified, and the similarities minimized. In scientific logic there is no sharp line of division between deduction and induction, nor is it possible to make observations which are wholly free from inference. It would be a rash scientist who would maintain that his observations contain no inferential material, and a bold logician who would offer to separate cleanly all deductions from inductions in scientific reasoning. Not for nothing, let us hope, has *Gestalttheorie* laid such heavy stress on the inseparability of 'parts' out of a whole. When therefore it is now urged that observation and inference are more important at the present time for scientific psychology than deduction, the thesis should be interpreted as one of emphasis, not of mutual exclusion.

Operationism has little to offer psychology, or any other science, with respect to the problem of observation. Everyone at all familiar with science is fully aware of the strict insistence, amounting to a point of honor, which is always placed upon the exercise of every possible precaution in the collecting of observational material. For the man in the laboratory this ancient and honorable tradition serves to take care of the problem of observation. He is familiar with the methods of concomitant variations, he devises ways and means of controlling and varying his conditions of observation in order to find out which conditions are important, which ones are irrelevant; he spends a good deal of his time experimenting with apparatus intended to isolate and sharpen the observations; and he knows the necessity of checking and rechecking his work. If his published results stir up a controversy, ninety-nine times out of a hundred such a controversy is due to the *interpretation* of the results, not to the

results themselves. When Carlyle heard that Margaret Fuller had learned to accept the world he remarked, "By Gad, she'd better." So with a scientific observation, the only thing to do is to accept it. If an experimenter reports that after fifty trials all but one of his twenty-five white mice run a maze without turning once into a *cul de sac,* there is no reason to doubt the observation. If he says that nine of his ten observers report the presence of green in the visual field after the removal of a red object which had been fixated for twenty seconds, again there is no reason to question the observation.

Acceptance of a scientific observation carries with it no obligation to use it. A given set of results may be correct enough, but of negligible value, except possibly to the person who collected them. At the moment an electric wire is swaying gently in the breeze outside the window. A report on the number of swings it makes in a given unit of time would hardly deserve publication in a scientific journal. Nor does acceptance of a result mean that it is to be regarded as permanent. Different methods may change the result, and lend to it more crucial significance. In its first form it is still correct, but it has lost its importance. It has been superseded by results secured under more accurately controlled conditions.

UNWARRANTED EXCLUSION OF DATA

These comments about initial data and their observation go almost without saying, save for one perverse exception which appears only in psychology. All that operationism can do is to say the same unnecessary things all over again. The exception, in psychology, relates to the strange proclivity on the part of some psychologists to reject certain observations, not because of any inaccuracy which they contain, but because

they do not happen to fit in with certain preconceived notions as to what constitutes the proper subject-matter of psychological science.

The views of the logical empiricists and of the operationists have already been seized upon with avidity by a number of psychologists.[31] The confusion which has prevailed in psychology in recent years has made more acute than ever the need for some sort of logical device which might help to straighten matters out. Since operationism purports to be just such a device, it is not surprising that psychologists should turn to it for assistance. Neither is it surprising, in view of the difficulties which psychology has had with definitions of subject-matter, that an appeal should be made to operationism in defense of this or that particular idiosyncrasy of definition. But every appeal of this sort, if made in the hope of retaining one kind of subject-matter and eliminating another, is bound to fail, for it is just here that operationism, along with every other logical tool, no matter how sharp, is helpless.

[31] See, for example, R. Carnap, Psychologie in physikalischer Sprache, *Erkenntnis,* 1932/33, 3, 107-142; K. Duncker, Behaviorismus und Gestalt Psychologie (Kritische Bemerkungen zu Carnap's "Psychologie in physikalischer Sprache"), *ibid.,* 162-176; H. Feigl, Logical analysis of the psychophysical problem, *Philosophy of Science,* 1934, 1, 420-445; E. C. Tolman, Psychology versus immediate experience, *ibid.,* 1935, 2, 356-380; Operational behaviorism and current trends in psychology, *Proceedings of the Twenty-fifth Anniversary Celebration of the Inauguration of Graduate Studies at the University of Southern California,* 1936, 89-103; S. S. Stevens, The operational basis of psychology, *American Journal of Psychology,* 1935, 47, 323-330; The operational definition of psychological concepts, *Psychological Review,* 1935, 42, 517-527; Psychology: the propaedeutic science, *Philosophy of Science,* 1936, 3, 90-103; in K. Koffka's recent book, *Principles of Gestalt Psychology,* 1935, may be found, scattered throughout the work, a number of strong objections to positivism in psychology.

Ontological differences in scientific subject-matter do not exist. The starting point of all science is a protocol containing the verbal report of some event which an observer has been set to watch. The metaphysical status of the events recorded in such protocols is beyond the possibility of science to determine. Acceptance of selected events as legitimate subject-matter for psychology, and rejection of certain others, must therefore be based on some philosophical prejudice, not on any operational criterion. Such was the argument of the first chapter, and the thesis there presented may be recalled briefly in connection with renewed efforts in certain quarters to purify psychological subject-matter by means of operational criteria.

OBJECTIVE VS. SUBJECTIVE

Throughout the arguments which Tolman has recently advanced in defense of behavioristic subject-matter there occur numerous references to the requirement that psychological data must be *objective.* Psychology seeks "the *objectively* stateable laws and processes governing behavior. . . . The laws and processes determining this their behavior are stateable in *objective* terms. Even in the cases where the organism is oneself, these determining factors can and must—for the purposes of psychology—be stated *objectively.*" [32] It is desirable to find out what meaning can be given to the word 'objective,' for in behavioristic writings a good deal of implicit thinking appears to be borne blithely along by some cherished meaning of the word which unfortunately is rarely made explicit. "I shall hold," says Tolman, "that immediate

[32] E. C. Tolman, Psychology versus immediate experience, *Philosophy of Science,* 1935, 2, 356. Italics mine.

experience just as it appears, contains quite as much objectivity as it does subjectivity. Immediate experience, as initially given, is not my private world or your private world. It is not something to be studied primarily by psychology. It is, rather, an initial, common matrix out of which both physics and psychology are evolved. It is the only tangible real that we have." [33] Here Tolman would seem to be defending a position similar to the one outlined in these pages, especially in the first chapter. The materials of scientific observations all come from the same source. Divisions of these materials into physical, biological, or psychological data are merely for convenience of reference. Basically there are no differences of kind among them. Yet in the very first sentences of his article Tolman writes, "In this paper I am going to try to indicate my notion concerning the nature and subject-matter of psychology. I am a behaviorist. I hold that *psychology does not seek descriptions and intercommunications concerning immediate experience* per se." [34] How are these two sets of statements to be reconciled? On one page one learns that psychology and physics both tap the common matrix of immediate experience for their materials, but on another page one reads the flat pronouncement that psychology has no traffic with immediate experience.

If by immediate experience is meant anything tangible enough to be reported with decent accuracy and consistency by men trained in scientific observation, then it is obvious that all science starts with such experience and also returns to it to test the validity of its concepts. Behaviorists know this as well as anyone else, but it would take a good Freudian to

[33] *Op. cit.*, 359.
[34] *Op. cit.*, 356. Italics mine.

discover why, knowing it full well, they nevertheless refuse to admit it. Terrified lest the purity of their subject-matter be defiled by the "raw feels" of classical psychology, they close their eyes and run as fast as they can, hoping to land in the verbally comfortable lap of objectivity.

The Shorter Oxford Dictionary defines 'objective' in nine different ways. Such wealth of meaning is embarrassing, and since behaviorists generally do not relieve the situation by confessing their own choice of meaning, the only thing to do is to make a guess. In many contexts the word refers to objects which are supposed to exist apart from anyone's knowledge of them. Such a supposition is a metaphysical faith, and is altogether too treacherous to use as a basic assumption in science. Surely critical behaviorism can not afford to take up with a view which can so easily be turned inside out.

A second common meaning refers to the *attitude* of the person making an observation rather than to the object of his observation. If an observer is detached, impartial, free from bias, ready to accept facts just as they are, he is said to be objective. The meaning is clear enough, but it can not serve as a criterion for distributing scientific materials into different compartments, for it does not refer to the materials themselves, but only to the attitude taken toward them. All scientific observers are objective in this sense, or at least try to be. Classical psychologists were not one whit less objective toward their work than are the psychologists of today, whatever their brand. Lurking somewhere in this meaning of the word it is occasionally possible to detect a trace of snobbishness—not in Tolman's use of it, but certainly in the connotation which some behaviorists give to it. Does not behaviorism possess, because of its insistence upon objectivity, a greater

degree of *accuracy* in its results? It may be granted that classical psychologists were well enough intentioned, but their tender love for images and sensations led them into poetry and romance, so it is said. Well, who can decide the matter? Surely not the psychologists, for they are prejudiced. Let an impartial jury be called in to examine the work of two great scientists, one of whom spent a good part of his life studying 'subjective' sensations, the other studying 'objective' reflexes. Let the two men be Helmholtz and Pavlov. Whose work is more objective in this second sense of the word? Whose work is more accurate?

The third meaning which 'objective' frequently seems to carry in behavioristic writings is harder to get at, partly because it may so easily be confused with the first meaning above, and partly because it is so seldom explicitly defined. The first meaning of the word is metaphysical: the assertion that objects exist independently of any knowledge about them. It is not metaphysics, however, to assert that an object is "out there," meaning thereby that the perceived space occupied by the object does not coincide with the space occupied by the body of the person who reports perceiving the object. No metaphysical status is ascribed to the object. The "out there" merely assigns to the object a position in space; a position, moreover, which is declared to be different from the one bounded by the surface of the observer's own organism. The declaration can be given operational meaning by noting that either the object or the observer, or both, must change their position before the two can make contact with each other.

Objects or events which are objective in this sense of the word appear to be regarded with genuine affection by behaviorists. The rat running in a maze is perfectly objective.

There he is, right "out there." Anybody can see him. But apparently the *color* of the rat (=sensation) is not out there on the rat. It is inside the observer, conscious, subjective, elusive, full of raw feel, visionary, illusory, romantic, and all that sort of thing; in short, wholly unsuitable stuff to make a science out of. Objects and their movements are objective, and are therefore legitimate subject-matter for science; but their surface-appearance, their sensory qualities are subjective, and for that reason must be excluded from science. Such would seem to be the droll view of things as seen by behaviorists. The view could be dismissed as so much palpable absurdity, were it not for the fact that it is taken seriously by such a large and important section of modern psychology.

Let it be granted, as indeed it must be, that a rat is objective in the meaning of that word now under consideration: its location in sensory or perceived space is external to the observer. Whatever the observer reports about the rat must be based on visual sensory data. The rat, as an object of scientific observation, is a visual phenomenon. The color of the rat is no less objective than the size or the movements. To be sure, some of the *conditions* for seeing color (retina, optic tracts, thalamus, *etc.*) are inside the observer and may therefore be called subjective; but equally subjective are some of the conditions for seeing a purely objective behavioristic rat, whatever such an entity may be.[35] The presence of subjective conditions does not deprive an event of objectivity. If it did, there would be no objective events anywhere in the world.

[35] The behaviorist's conception of a rat, or of any other object, resembles in many respects the philosopher's *Ding an sich:* a noumenal entity divorced from the everyday world of reality, non-palpable, non-sensory, non-existent.

All visual phenomena, in spite of their partial dependence on subjective factors, are objective, so that classical psychology, to the extent that it was interested in visual material, can not be accused of being any less objective than behaviorism.

Auditory phenomena are in this sense also objective: the sounds of an orchestra come from the stage or pit, not from inside the head of the listener. Pressures, pains, temperatures are subjective phenomena: they are localized within the space occupied by the observer's body. But psychological and physiological work on these bodily sensations shows no marked inferiority to work in other sensory fields. Their subjectivity has apparently not made their reportability any more difficult or less reliable than the reports made on visual and auditory phenomena. It is therefore not clear why the difference in their location in space should be sufficient cause for excluding them from science.

Unprejudiced consideration of scientific observation must make it clear that initial data are almost always sensory in nature. The most important and certainly the most frequently used sensory materials, namely, those from vision and audition, are objective. All scientists make use of them, physicists as well as introspectionists. Any attempt to discriminate among the sciences on the basis of the degree of objectivity of their initial data must eventually come to naught, although for considerable periods of time persuasive arguments may lead the unwary into supposing that color changes, for example, reported by a chemist are more objective than similar visual phenomena which happen to interest a psychologist, otherwise behaviorists could not have convinced both

themselves and others that classical psychology was dealing exclusively with subjective raw feels.

The limited range of sensory phenomena that are subjective has not been left solely to the psychologist to investigate. Sensations which are localized inside the body have been studied by medical science and physiology as well as by psychology, although the most exhaustive and systematic treatment of these phenomena is to be found in psychology. The same phenomena have been turned to very frequently, however, by various branches of biological science, but since the latter have not been particularly bothered by the so-called subjectivity of bodily processes like pain and the raw feel of temperatures, it seems unfortunate that psychology should work itself almost to the point of a nervous breakdown merely because a small number of its initial data have an internal rather than an external localization. The fact that they have an internal localization does not make them any more private than are the so-called external sensory materials.

All phenomena are private to begin with. They become public only when their initial privacy is put into words. A rat in a maze can be as private as a pain in the stomach. The warmth of a fire can be as public as the red glow of its flame. It all depends on whether or not such data are observed with an intent to give them verbal formulation. Whether they can be adequately *reported* is the only question which concerns science. Their apparent localization in space, that is, their subjectivity or objectivity, can furnish no adequate basis for their evaluation as scientific facts. Any phenomenon becomes fact if it is consistently reported by scientific workers who are interested in it and remains fact until more accurate meth-

ods of observation lead perchance to contradictory reports. Whether the phenomenon is objective or not is wholly irrelevant to its status as scientific material. If, therefore, by objectivity Tolman means the *consistency* and *accuracy* with which data are reported, he would be hard pressed to demonstrate that behaviorism is more objective than classical psychology in this respect; if by objective he refers to the "out there" kind of phenomena, he would have difficulty in showing that behaviorism has exclusive claim to such phenomena, or that "inside" phenomena are less accurately or consistently reported.

In making initial observations for scientific use it happens all too frequently that different observers do not agree in their reports. At least two obvious reasons may be cited to account for such a disagreeable state of affairs. Part of the difficulty may be due to basic ineradicable differences among the observers themselves. Ever since 1796, when the assistant in astronomy at the Greenwich Observatory was dismissed because his readings of stellar transits did not agree with those made by his chief, there has been a keen interest in what is commonly referred to as the *personal equation* of the scientific observer. In psychology an elaborate field of inquiry known as *individual differences* has developed out of this interest. Scientifically, however, it is lazy and dangerous business to fall back always on the assumption that individual differences in making a report are due to unavoidable personal equations of the observers. In many cases they are due to another, and because they can often be eliminated, a far more important reason: in making their observations the different observers are simply not observing the same thing. In the case of the Greenwich astronomers several events came

within the field of observation: the position of the hands of a clock, the sound and number of the clock-beats, the parallel cross-wires in the reticle of a telescope, and the spatial and temporal relation of these various items to the position of a given star as seen in the telescopic field. A difference in report could easily be brought about by a difference in the direction of concentration. If one observer concentrates more on the clock-beats, and another more on the cross-wires, the two may differ merely because they are not observing quite the same thing.

One hundred and forty odd years after the famous case of the Greenwich astronomer it ought certainly be platitudinous to insist that every observation must be the culmination of a method which is designed to eliminate as far as humanly possible all variation in report due to the fact that different observers are not reporting the same thing.[36] A report is always partially the function of the method of observation, so that if the method does not control or minimize ambiguous possibilities of observation, the reports are bound to be correspondingly ambiguous.

The most accurate type of report can usually be made when the field of observation is narrowed down to a single item of experience, or to two items, so that the observer is required merely to note whether or not the item is present, or, in the case of two items, what relation one item bears to the other

[36] Yet countless psychological experiments, so-called, are done today by means of a sort of shot-gun method. A complicated stimulus set-up, such as a picture, or a paragraph heard from a loud speaker, is presented to observers who are merely told to report what they see or hear. Almost anyone would know that under such circumstances the observers would hear or see different things. It takes psychologists to be impressed by such hackneyed stuff and to talk learnedly about its subsumption under a *theory* of individual differences.

in terms of some dimension like size, weight, length, brightness, pitch, loudness, *etc.* When the stimuli are controlled and ready, when the observer is practiced and fully prepared to make an observation, the task of describing the experience is made as restricted and univocal as possible for the observer by allowing him to cast his description into the form of a simple act of discrimination. By merely saying "yes" or "no," "lighter" or "darker," or simply by pressing a key with his finger, or by using any other prearranged form of symbolic signal, the observer may give public expression to the nature of his private experience. The discrimination is the *operation* by which the experience of the observer is made known to others. No one but the observer can know the experience directly. However, by rigid control of the situation in which the experience occurs and by strict methodological use of symbols of communication, the private experience may be embodied in a protocol which then becomes public property for the use of all interested scientific workers. The experience is defined in terms of the experimental set-up and the operations of report. The operations and the experience are not, of course, identical. The experience is merely symbolized and limited by the operations; but if the operations are accurately specified, the originally unique particular experience may be added to the fund of common universal knowledge—and thereby lose its solipsistic character.

THE DISCRIMINATORY RESPONSE

The undeniable importance of the discriminatory operation for locating and defining experience has recently led Stevens into the performance of an extraordinary act of so-

phistic prestidigitation. The very world in which we live, and move, and have our being, is made utterly to disappear—with the exception of one small item, namely, the discriminatory reaction on the part of a human being. It is not only psychology which is thus reduced to a state of bankruptcy. Not a trace of anything is left for any science to investigate, save the discriminatory reaction. There is not even anything left to discriminate. Only the tiny rack of discriminatory reaction remains. Nay, discrimination itself vanishes, for to know, and then only indirectly, that a discrimination has been made requires a further discriminatory reaction; but since the things discriminated by such a reaction can not be known, it must follow that discrimination is unknowable and therefore beyond the range of scientific inquiry.

"We need no longer think of immediate experience as the subject-matter of psychology and the basis of physics; instead we may say that the differential reaction makes physics possible and it is the business of psychology to investigate the nature of such reactions." [37] Colors, sounds, lights, odors, temperatures, and the varieties and shapes and movements of all objects of the whole great globe itself: these all fade and dissolve, leaving behind nothing more substantial for science than a differential reaction. For example, if by pressing a key with his finger an observer can indicate his ability to tell the difference between two wave-lengths, say in the neighborhood of 700 μμ, the initial data, in such a case, are the movements of the finger on the key, not the experience of two slightly different red colors. The finger-movements are ob-

[37] S. S. Stevens, Psychology: the propaedeutic science, *Philosophy of Science*, 1936, 3, 95.

jective and physical and therefore scientific; the colors merely the baseless fabric of illusion.[38]

The elementary discriminatory reaction on the part of human beings, then, is the fundamental operation of all science; and by discrimination is meant the concrete, 'physical' reactions of the organism to either internal or external environmental conditions.

In thus placing discrimination at the basis of all science we allow it to usurp the position formerly enjoyed by 'experience' or the 'immediately given.' *Does this substitution mean that experience and elementary reactions are equivalent? It does, precisely.* Any attempt to define the term experience operationally or point out what, concretely, is meant by the philosopher's 'given' discloses at once that the discriminatory reaction is the only objective, verifiable thing denoted. Scientific psychology is operational and as such can have nothing to do with any private or inner experience for the simple reason that an operation for penetrating privacy is self-contradictory.[39]

If to be objective in psychology means the adoption of such a point of view, one can only hope that operationism, instead of giving it strength, will cause it to languish and die. Better the rankest sort of subjectivism, whatever such an '-ism' may mean, than an objectivism which deprives all science of all subject-matter save discriminatory reaction. No metaphysical solipsism was ever so drastic in its nihilism. If there can be no operation for the penetration of privacy, the only trouble with Stevens' logic is that it is illogical, for it must follow from his argument that there can be no operation for the penetration of the initial privacy of discrimina-

[38] Like Tolman, Stevens is moved by the desire to make psychology objective. He does not make it clear, however, just what he means by objective. His frequent use of single quotes around the words 'objective' and 'physical' might be taken to indicate a measure of uncertainty in his own mind as to just what the words do mean.

[39] Stevens, *ibid.* Italics mine.

tion. If discrimination is to be used as a scientific datum, it can not remain in a vacuum. Someone must know about it. But the person who knows about it is in possession of private experience which, according to Stevens, is impenetrable. The objective discriminatory reaction is really subjective and can therefore not be used by science. The baby *must* be thrown out with the bathwater.

The attempt on Stevens' part to *equate* the baby and the bathwater makes it impossible to keep anything from disappearing. In answer to the question as to whether his view means that private experience and discriminatory reactions are equivalent, he replies that it does mean that, precisely. Having therefore argued that scientific psychology can have nothing to do with private experience he seems to have landed himself in the predicament of saying that since private experience and discriminatory reactions are the same thing, scientific psychology can have nothing to do with discriminatory reactions. Everything disappears.

The bizarre logic which enables Stevens to pull the whole temple of science crashing down about his head may be illustrated in another way. An elementary discriminatory reaction and the objects discriminated by such a reaction are one and the same thing. One may be substituted for the other, the preference in favor of the reaction being based merely on the supposition that it is objective and public, whereas the object discriminated by the reaction is subjective and private. Let the objects of discrimination be tonal densities (private experience), a field in which Stevens has done pioneer research, and let the manner of reaction be the pressure of the finger on a key (public operation). Tonal density and finger-reaction, it must be remembered, may be substituted for each

other, for they are equivalent. Now let the objects of discrimination be the ideas of Plato and of Aristotle. If a person can make distinctions among them, he signifies his ability to do so by pressing a key. Here again, the ideas of Aristotle and Plato are equal to a finger-reaction; the objects discriminated and the act of discrimination are identical. But a finger-reaction is also equal to tonal density. Therefore, in terms of Euclid's first axiom, Stevens has made the ideas of Plato and Aristotle, to all scientific intents and purposes, equal to so much tonal density.

DIRECT EXPERIENCE

Scientific psychology should indeed be operational, but to argue that on this account psychology must have nothing to do with direct experience is a strange misconception of one of the most important contributions which operationism can possibly make to scientific methodology. The initial material of all science is private: the immediate experience of some human being is the starting point of every scientific inquiry. To make the initial private experience public requires the use of some symbol which shall *mean* that experience to other interested people. If an operation for penetrating privacy were self-contradictory, no intelligible communication of any kind could ever take place between two individuals. All symbols, all gestures, all language of every kind would be meaningless. There could be no such thing as science. Each individual would organize his own experience according to his own private fashion, and it would remain forever impossible for him or anyone else to discover whether two people could know the same thing. Since such impenetrable privacy is manifestly and fortunately not the rule of human life, it must

mean that operations *do* exist which make it possible to bring private experience out into the open. And one may readily grant that Stevens is right in his insistence that the discriminatory reaction is one of the best and most conveniently handled operations for locating and defining an experience. It is only one, however, of several operations and conditions which are essential to the proper definition of an experience for use in science.

If, for example, tonal density is the subject of investigation, a number of conditions must be specified before any operation can be given univocal meaning: precise statement of the stimuli and their mode of presentation, amount of preliminary training on the part of the observer, the instructions given to the observer (in which he may be told to press a key if he can tell the difference between two sounds with respect to their density), number of times the stimuli are presented, the consistency of the observer's reports and their agreement with reports given by other observers, *etc.* Taken in relation to all of these conditions the discriminatory finger-reaction then, and only then, means a definite difference in tonal density. Under other conditions the same reaction might mean something totally different. But under the former conditions it means tonal density, and the results secured are regarded as a contribution to the psychology of that topic. They are so interpreted by everyone who reads them. Only the author, in case he happens to be Stevens, would entertain the view that the results are a contribution to the psychology of finger-reaction. Yet if Stevens regarded tonal densities and finger-reactions as equivalent, any physiological hypotheses he might propose would be as valid for an explanation of digital movement as they would be for tonal phenomena; but

it is clear from his own work that such experiments are designed to study physiological acoustics, not the physiological mechanism of motor reactions.

Any given operation like a discriminatory reaction, together with its surrounding conditions, can only serve to *define* an experience. It can not take the place of the experience. It points to, limits, and conceptualizes the experience so that it may become a public datum. What the experience is like apart from its definition can never be known intimately, except by the person who has it. But this fact does not mean that the definition of the experience, rather than the experience itself, becomes the datum. Nor does it mean that one must know the experience directly and intimately in order to understand its definition. It is a simple enough matter to define the other side of the moon. A person who has never heard the partials of a complex tone could nevertheless understand their definition. Yet it would be impossible to build a science of overtones out of operational definitions, just as it is still impossible, merely with the aid of a definition, to observe the other side of the moon.[40] In scientific work all observational data are inseparably linked with their definitions, but they are not on that account identical with their definitions. A scientific *concept* should be neither more nor less than its definition. Definition and concept are identical, or should be; but the observational data drawn upon to support the definition of a concept can not themselves be merely definitions, otherwise the validity of a definition could be tested not by a return to observation and fact, but only by

[40] The epistemological problem of the relation between sense data and their definition has been subjected to sharp analysis by C. I. Lewis, *Mind and the World Order,* 1929, 67-89.

verbal analysis of a further definition, which definition in turn could only be tested by an additional definition: an endless regress which would turn the serious game of science into a mere play on words.

The value of operationism in psychology, as will be seen more clearly in the next chapter, lies in the assistance which it can give in the construction of concepts and theories. It is here, in the field of inference and induction, that the bad pitfalls of psychology are to be found. At the level of initial observation the pitfalls are not nearly so dangerous, and they can usually be avoided, insofar as it is humanly possible to avoid them, by exercising the precautions which science has always insisted upon in its gathering of factual data. All that operationism can do at this level is to reiterate what has already been said many times before in the history of science in an effort to clarify the *methodology* of fact-finding. But if at this level operationism tries to convert a methodology into a *metaphysics,* it will achieve thereby its own inglorious defeat. The operationists, or at all events Bridgman, and even more especially the logical empiricists, have argued that strict adherence to their tenets will serve to rid science of many, if not all, of its insoluble metaphysical problems. Yet it must be obvious that attempts to eliminate certain kinds of data from observation, on the grounds that they are too subjective or insufficiently objective, will bog psychology down into a morass of ontological nonsense. The fundamental *nature* of an observational datum is of no concern to psychology. Only the consistency with which it is reported, which is a purely methodological question, has any relevance.

CHAPTER V

Psychological Physiology

IN THE PRECEDING CHAPTER two aspects of scientific methodology, the collecting of data and the testing of deductions, were examined in the light of operationism. Neither of these procedures can derive much assistance from operationism. With respect to fact-finding, or more correctly, the collecting of initial data of observation, operationism has nothing to offer.

With respect to testing the validity of deductions, operationism encounters difficulty because of the present status of psychology itself. Large generalizations that permit the deduction of important consequences are rare in psychology, or rather, they are so likely to be ambiguous in meaning that it is not possible to know whether a given deduction is a logical consequence or not. Psychological events are so complexly determined and therefore so difficult to control that it is seldom safe to generalize them beyond the conditions under which the observations and experiments were made. It is a reasonable guess, for example, that the concept of *determination,* as worked out years ago in sensory psychology, has much in common with that of *motivation,* as developed in modern studies of complex social phenomena. Yet only a rash theorist would attempt to argue that these two concepts, each of which is ambiguous enough by itself, are logically

interchangeable. The theoretical interests which they serve are still so far apart that it would be little more than a play on words to combine them into a single principle.

Nearly all psychological concepts are ambiguous. They contain a core of meaning which is undoubtedly understood in the same way by nearly everyone and which could probably be given strict operational definition, but into the periphery have crept meanings that are hazy and elusive, meanings which can not be identified with any observations or logical operations. It is because of this fact that psychology needs the assistance of operationism in the construction of concepts. Between the initial observations and the inferences which lead to generalization many a logical slip may and usually does occur. The fuzz which surrounds psychological concepts can be illustrated by any random list. Who knows the precise meaning of perception, drive, imageless thought, mass action, instinct, learning, insight, or isomorphism—to cite the first few that come to mind?

REIFICATION

The construction of absolutely pure operational meanings, to be sure, would be practically impossible, even if it were desirable. Purity of meaning is a matter of degree in which the last stage of perfection is always a goal, never an actuality. All that constant vigilance can do is to keep meanings from slipping back into a stage of serious ambiguity. Every science, but especially psychology, is confronted with certain almost ineradicable habits of thought. These habits, if left unchecked, work no end of logical mischief; if brought out into the open and watched, their dangerous influence may be counteracted. Some of these modes of thought have already

been referred to in a previous section.[41] In the present context two of them are particularly cogent, and may be called, for convenience of reference, *reification* and *infiltration.*

Scientific observations and experiments are rarely confined to the examination of an isolated example of a given event. The relation between the sensory items which constitute the 'event' is observed at different times and in different places, and the same relation obtains even when the sensory items themselves are not the same. A familiar phenomenon in color vision may serve as an example.

If concentric bands of uniform color, say red, are separated on a disk by a narrow band of gray, and if the disk is made to rotate rapidly, the narrow band looks decidedly greenish instead of gray. An exact statement of the conditions under which such an observation is made would have to give specification of stimulus variables (frequency, amplitude, and form), including those to which the induced green is matched, surrounding illumination, width of bands, distance of the observer from the disk, and as much as possible about the optical system of the person making the observations. Specifications of this sort would run into a paragraph of several sentences, and meticulous accuracy would require a repetition of the whole paragraph every time the observation came up for discussion. Only in this way could one person be sure that he was making the meanings of his spoken or written words as clear as possible to another person. Yet it is obvious that even scientific discussion and writing can not afford to tolerate quite such cumbersome verbiage. A short cut is found by substituting a single word, or a brief phrase, for the detailed description of the phenomenon and

[41] P. 67 f.

the specification of its conditions. In the above example the word is *contrast,* and if the word is taken to mean the initial observation and the procedures followed to make possible the placing of the phenomenon in a reproducible setting, there can be no reasonable objection, on operational grounds, to the use of the word, even when it comes to mean all possible induced colors in a variety of different settings, provided it is possible, when challenged, to produce the specifications.

The very fact that contrast refers to many combinations of colors and settings means that the word must be taken as a concept, not as a thing. There is no such *thing* as contrast. No one can *see* contrast, for contrast is merely a word. What one sees is a pattern of colors, which still remains a pattern of colors no matter what words are used. It is extremely difficult, however, not to think of a scientific concept as a thing. In the case of contrast the dangers of such reification are not great, for in the minds of most psychologists the meaning of the concept is closely linked to the color phenomena themselves. But in the case of many concepts in psychology the tendency to reification may lead to serious misunderstandings and confusion.

An example of such confusion may be found in the present controversies in psychology with respect to the relative merits of the concepts of *type* and *trait* in the study of personality.[42] At the hands of most writers both of these concepts have been removed from the vulgar arena of concrete human behavior, and reified almost beyond recognition.

> Again we refer to the sharp contrast between the theory of traits and the doctrine (any doctrine) of types. Unlike traits, types always have biosocial reference. A man can be said to *have* a trait; but he

[42] G. W. Allport, *Personality,* 1937, 286 ff.

can not be said to *have* a type. Rather he *fits* a type. This bit of usage betrays the important fact that types exist not in people or in nature, but rather in the eye of the observer. Type includes more than is in the individual. Traits, on the contrary, are considered wholly within the compass of the individual. The crux of the distinction is that in type the reference point is always some attribute, or cluster of correlating attributes abstracted from various personalities, a biosocial reference defined by the interest of the particular investigator.

Many kinds of typology flourish. There are literary types, energy types, pathological types, constitutional types, eidetic types, statistical types, and ideal types. Whatever the kind, a typology is always a device for exalting its author's special interest at the expense of the individuality of the life which he ruthlessly dismembers. Every typology is based on the abstraction of some segment from the total personality, and the forcing of this segment to unnatural prominence. All typologies place boundaries where boundaries do not belong. They are artificial categories.[43]

These harmless strictures against typology could be directed with equal irrelevancy toward any kind of traitology, or for that matter, toward any concept anywhere in science. All scientific concepts are abstractions and therefore inevitably possess a certain element of artificiality. There is no such thing, for example, as height apart from concrete objects, yet all sciences quite rightly treat spatial dimensions *as though* they had independent existence.[44] All scientific concepts, since they are based on selected items of observation, create boundaries and achieve a prominence not present in

[43] *Ibid.*, 295f.

[44] The Gestalt psychologists have always tried to insist that *Gestalten* are more real than *sensations*. Both of these concepts represent artificial constructions, to which degrees of reality can hardly apply. The question therefore as to whether a *Gestalt* is more real than a *sensation* is not a meaningful one.

the original welter of behavioral data. Neither traits nor types, as concepts, have any real existence. They are merely words, and words do not exist in the eye of the observer nor in the people observed. A man can not be said to *have* either a type or a trait, but he can be said to *fit* either a type or a trait. At present the fit will be inexact, for dimensions of personality have not yet been quantified well enough to permit of accurate measurement. In the case of height, the measurement can be precise, and little confusion results from saying that a man *has* a certain height. Observation and concept are so closely related that the phrase is not ordinarily understood to mean more than it says, namely, that the extent of a given datum of observation in one direction fits a certain section of an ideal dimension of distance. But if an attempt is made to fit some mode of human conduct to the trait of courage, the looseness of correspondence between behavior and concept leads to mischievous reification. The concept parts company with behavior, picks up undefined notions in its flight from reality, and finally acquires an independent real existence in its own right, so that when it is said that a man *has* courage, he will be thought of as the fortunate owner of something considerably more significant than a certain pattern of behavior.

The psychological logic involved in the construction of scientific concepts is tricky and elusive. The only real facts are the initial data of observation, but these data, especially in a tremendously complex and relatively new field like personality, are always too numerous and varied for individual specification. They must be thrown into groups, on the basis of some common characteristic, and then given a verbal label: attitude, directional tendency, generalized habit, in-

terest, mode of adjustment, need integrate, sentiment, trend, value, trait, type—the label itself is not significant. Abracadabra or hocus-pocus would be perfectly all right. Whatever the word or phrase, it must *mean* the initial observations, plus all the operations involved in making the observations. Now if the meaning comes in the course of usage to take on the characteristics of a concrete thing, a logical fallacy is committed, for a concept can not be identified with any definite thing. It is a verbal summary of many aspects of things, aspects which have been artificially and deliberately isolated from their thing-contexts for the purpose of unequivocal and undisturbed inspection, a procedure which just as deliberately involves the artificial exclusion from observation of other aspects of the same things. The verbal tag used to denote these observed aspects has no concrete existence anywhere in the original facts or data. Color contrast can not be identical with red and green, for it must also be blue and yellow; but it can not be identical with blue and yellow, for it must . . . *etc.*

The meaning of any given concept is extremely precarious and difficult to fix, for it lies midway between the data of observation, in terms of which it must be defined but with none of which it can be specifically identified, and the veiled reifications of human imaginings, with the vagaries of which it must not be fused. It is part observation, part construction—an anomalous position fraught with everlasting danger to clear thinking.

INFILTRATION

Closely related to reification is a tendency in dealing with concepts which may be called *infiltration.* Meanings which

are not always made consciously explicit frequently seep into concepts. Even when concepts are given exact operational definitions, they run the risk of collecting increments of meaning from a variety of different sources. Most scientific workers, possibly the majority, do not restrict their use of concepts to operational definitions. Conceptual summarizing of factual material is only one part of the scientific game. Indeed, it is only the beginning. Facts cry out for explanatory hypotheses, and "an hypothesis," to quote J. S. Mill, "being a mere supposition, there are no other limits to hypotheses than those of the human imagination; we may, if we please, imagine, by way of accounting for an effect, some cause of a kind utterly unknown, and acting according to a law altogether fictitious." [45] As soon as any attempt is made to account for the factual material summarized in a concept, the chances are nearly inescapable that the concept will take on some or all of the meanings contained in the explanatory hypotheses. As the hypotheses move further into the realm of sheer speculation, the concepts become progressively more detached from their operational supports, which is another way of saying that they thereby become more and more meaningless.

Any guess, hunch, hypothesis, or theory that suggests itself as a possible explanation for the data used in defining some concept is more than likely to attach itself to the original operational meaning of the concept. It makes little difference how clearly or how vaguely formulated the additional increments may be. Once thought of, they constitute a dangerous source of ambiguity in scientific discussion or writing. As long as the increments remain locked up in the

[45] J. S. Mill, *A System of Logic,* 1843, Bk. III, Ch. XIV, par. 4.

mind of one individual, they contaminate only that one individual. But as soon as he opens his mouth, his disease will spread to all of his listeners. If the ideas are written down and published, the scientific world interested in them is then liable to infection. The infected ideas have two aspects: their own native impurity, plus all the additional impurities arising from mistaken interpretations in the minds of readers and listeners.

The concept of contrast, for example, may be defined operationally without much difficulty. For decades, however, psychologists and physiologists have discussed the concept and have tried to fit the facts subsumed under it into a larger framework of visual theory. The literature of the subject is enormous. What does the concept now mean? The question is a perfectly fair one in view of the variety of meanings which have filtered in during the years, and it is a fair guess that many an expert would be hard pressed to find a satisfactory answer. He would have to clean his mind of all accessions which have come from his intensive preoccupation with the topic, obviously not an easy thing to do, and go back to first principles, which in this particular matter he has probably not bothered to do for a long time. He may be convinced that he knows well enough what contrast means, but what he may not know is that his understanding of it, on account of its departure from the original operational meaning, does not coincide with interpretations held by other experts. When such experts disagree, the cause may frequently be found in the bits of nonsense which each one has more or less unwittingly introduced into his treatment of the topic.

If infiltration can operate upon a concept as exact as contrast, its pernicious influence must be ever so much more far-

reaching and insidious in a subject like traitology. Work on vision and audition is insured against serious chaos and nonsense by the precision of its initial observations. Tangled disputes in these fields can usually be unraveled by tracing concepts, whenever they are seriously challenged or confused, back to laboratory findings and procedures. Whenever backward tracing of this kind is successful, conflicts in theory are bound to disappear, if for no other reason than that they are seen for what they truly are, namely, nonsense. As Stevens and Davis sagely remark in the preface to their recent book on audition, "Theories flourish on a certain sparseness of facts and wither in the face of abundance. When all the relations are known, alternative theories are no longer possible, and, if a present inventory of the facts of audition leaves little room for theories of hearing—in the nineteenth-century meaning of that phrase—that situation must be accounted a sign of progress." [46] In traitology no such safe retreat from verbal confusion can be found.

Where does the basic material of traitology come from? Very little of it comes from controlled laboratory observation. Some of it is borrowed from the accumulated wisdom, if such it may be called, of common sense. A goodly portion of it comes from the operationally dubious procedure of the questionnaire. And a great deal of it comes from armchair speculation, which is certainly no better and perhaps no worse, for scientific purposes, than the imaginative and intuitive descriptions of human nature which poets and novelists have always woven into their literary creations. Whether such creations are scientifically true or not is impossible to say. Fidelity to nature is not a criterion of aesthetic excellence, al-

[46] S. S. Stevens and H. Davis, *Hearing*, 1938, x.

though the two qualities may often appear to go together, as when it is said that the writings of men like Tolstoy or Proust are profoundly true. Men who write in such vein have neither the patience nor detachment required to test the validity of their intuitions. Allowing their fancies to take flight without attentive concern for the exact nature of the starting points, they would too often find themselves, if confronted with a challenge for operational definitions, in the predicament of Browning who, when asked what some of his lines meant, had to confess that when they were first written he and God knew their meaning, but that now the secret belonged to God.

These comments are not intended in any sense as a reflection on the quality of work being done by psychologists who are trying to bring order into the discouragingly complex field of human personality. Work in this field is pursued with the same high intensity of scientific purpose as in any other field of inquiry. It must nevertheless be recognized that through no fault of the investigators the concepts in many branches of psychology, of which personality is an outstanding example, are still irritatingly ambiguous.

These wily processes of infiltration and reification are not always so hazardous as the strictures in the preceding paragraphs would seem to imply. In psychology they have an important and desirable role to play, and if they can be freed of their equivocality of meaning they deserve deliberate cultivation in the construction of many concepts. In this circumstance lies an apparent paradox. The following paragraphs are designed to resolve, if possible, this seeming inconsistency.

HYPOTHESES

Not many psychologists are able to rest content with purely operational definitions of their concepts. The absence of systematic uniformities in the observations summarized by the concepts creates the feeling that something is disturbingly incomplete. Point-to-point or one-to-one correlations between observed items in psychology are rare indeed. Imperfect correlations are the rule, with the result that the basic notion of a scientific law, namely, if *a*, then *b*, is seldom capable of demonstration. Generally the results only warrant some such statement as, if *a*, then perhaps 65 percent of the time *b*, but the rest of the time possibly *c, d, e, f, etc.* Scientifically this state of affairs is not a happy one, but in most branches of psychology it can not at the present time be avoided. This is why the literature of psychology is cluttered with more than its share of elaborate guesses designed to fill in the yawning gaps of knowledge that are thrown into prominence by imperfect correlations.

Now the making of guesses is by no means an unmitigated evil in science. Science thrives on guesses and would make slow progress without them. The great trouble is that in psychology too many guesses turn out to be duds. The remedy for this state of affairs can be found not by placing any sort of ban on guessing, but rather by trying to discover ways and means of improving the quality of the guessing. Here is where operationism can be of practical value, namely, by placing a ban on the sort of guessing which has nothing to recommend it other than its guess-like quality. Scientific imagination needs to be held in restraint. Hypotheses that

run out to the limits of human imagination are seldom of any value, as Mill hastened to point out in the sentences immediately following the famous passage quoted above. Since "hypotheses of this sort would not have any of the plausibility belonging to those which ally themselves by analogy with known laws of nature, and besides would not supply the want which arbitrary hypotheses are generally invented to satisfy," he believed, perhaps too optimistically, that examples of their use by reputable scientists would be rare.[47]

The very natural and understandable desire to make use of guesses to fill up gaps in knowledge encounters certain hazards in psychology which are present to a greater or less degree in all science, but which reach their maximum in the study of human behavior. Bridgman in his writings on operationism frequently mentions the tendency on the part of physicists to use phrases in their writings which imply animistic powers in the causal connections of nature. Forces and energies attract and repel objects. Many concepts in physics reveal this tendency: impulse, stress, tension, resistance, *etc.* Their use represents for the most part mere linguistic heritage and habit, but even in modern physics, according to Bridgman, there is still danger that these verbal concepts will be thought of as reified causes.

DYNAMIC PSYCHOLOGY

This animistic tendency runs riot in psychology, and seems to show no signs of abatement. On the contrary, judging from the popularity of so-called dynamic psychologies, the tendency is today livelier than ever. The history of psychology is shot through with animistic concepts: soul, will, fac-

[47] J. S. Mill, *loc. cit.,* footnote p. 117.

ulty, creative synthesis, attention, volition, conation, intent, psychic energy, determination, purpose, need, wish, motive, incentive, urge, drive, *etc.* Every one of these concepts, and many others like them, have loomed large in psychology at one time or another, but it may seriously be doubted whether the vast amount of ink and cerebration devoted to them can do any more than befog issues until it is clearly understood that at present no meaning can be assigned to these concepts that has any dynamic explanatory power.

To say that a given bit of human action is due to a purpose or a need or any other kind of faculty is no more dynamic than to say that a startle-reflex is due to a loud sound. Yet the supporters of dynamic psychology insist that it is they, in contrast to all other psychologists, who are trying to solve the *why* of human behavior—as though that were a unique platform for a group of scientists to stand on! All science is interested in the *why* of the world in which we live. But the most elementary principles of scientific logic have long since made it clear that a scientific *why* can be no more than an exact *what* and *how*. If by *why* dynamic psychologists mean hidden powers behind the scenes that push and pull the human organism around, their search for the *why* will be in vain. For science there can be no hidden forces. Everything must be in full view and must be accurately observed and quantified. The objections frequently expressed by dynamic psychologists against coefficients of correlation indicate either ignorance or misunderstanding of the aims of science.

All scientific explanation, as will be shown in greater detail in the next chapter, turns out upon logical analysis to consist of networks of correlations. Operationally it is not

possible to show that one type of correlation is more or less dynamic than another. All correlations are based on observed or assumed uniformities between events, the antecedents in the events being called the cause, and the consequents, the effect. The correlation between knee-jerk and stimulus applied to patellar tendon is no less dynamic than that between some compulsive form of behavior and an assumed desire for inviolacy. Nor is the correlation between a given memory and some hypothetical active stress within the ego that is said to pull the memory out any more dynamic than that involved in saying "chair" when asked to reply with the first word that comes to mind on hearing the word "table." Dynamic psychologists seem to entertain the naïve belief, or rather, they are entertained by the belief, that the kind of correlations in which they are especially interested acquire dynamic qualities (whatever these may be) as the result of some sort of verbal magic.

In the above illustrations it will be noted that two of the correlations are observed (knee-jerk and paired associates), while the other two (desire for inviolacy and mnemonic neural stress) are assumed. Observed correlations are naturally more satisfying than those based on guesses, and in the earlier and more innocent days of psychology it was hoped that experimental analysis would produce a network of observed correlations large enough to reveal an inherent set of explanatory principles. In the more important fields of psychological inquiry, however, this hope has not yet been capable of realization. Guesses have to be made, and the dynamic psychologists, for whom, be it said to their credit, the most difficult and important fields of psychology seem to hold no terrors, are courageous enough to risk their scientific reputations

by making them. The difference in practical significance between the observed correlations to which psychologists can point with pride and the assumed correlations which they tend to view with alarm is considerable—from the point of view of society in general. The former, based largely on studies of sensation and reflex action, do not meet the urgent needs of a confused society, whereas the latter, abounding in words like repression, super-ego, regnancy, and dynamic stresses, seem full of vital significance. This important practical difference, however, has no basis whatever in any logical difference in the types of correlations. Logically they are indistinguishable, except to dynamic psychologists, who scornfully dub observed correlations static, cold, mechanistic, and artificially analytic. Applied to correlations these words have little meaning. Psychologists who use them in this context are talking nonsense.

In the construction of concepts, however, the difference between observed and assumed correlations needs to be kept carefully in mind, for in this difference lies partially concealed the crux of the paradoxical problem of the role of reified meanings.

INFERENCE FROM OBSERVATIONS

The fields in which psychological correlations most nearly approach unity are those of the simpler sensory and reflex phenomena. Studies of sensation, following the pioneer work of Helmholtz, were vigorously pursued by the Wundtians during the latter part of the last century. Relations between stimuli and various sensory dimensions were carefully analyzed, and numerous correlations were found to hold with fairly high uniformity. Analogous investigations of reflexes

were being carried out by Bethe, v. Uexküll, Loeb, and other early students of animal behavior. At the turn of the century it was patently obvious, however, that even these correlations were not stable enough to permit generalizations that could be dignified by the name of laws. At practically the same time two men were beginning to insist in their writings that between stimulus and sensation or between stimulus and response a third intermediate factor must be postulated in order to account for the variations in the observed items. On the basis of his famous abstraction experiments, Külpe developed the notion of *Aufgabe* or mental set in an effort to make intelligible the shifts in sensory attributes in relation to a constant stimulus.[48] Given the same *Aufgabe,* it was then possible, he argued, to demonstrate a constant relation between the stimulus and a single sensory attribute. Practically the same position with respect to reflex action was defended by Jennings in discussing his experimental work on lower organisms.

> The importance attributed to the concept of reflex action is of course due to the desire to find a simple invariable unit for behavior, comparable to the atom in physics. To obtain such a unit it is necessary to take into consideration as an additional possible variable, the physiological state of the organism . . . We cannot possibly say for a given organism "same stimulus, same reaction," as appears to be the usual idea of a reflex. On the other hand we can say "same physiological state, same stimulus, same reaction," and this supplies whatever need there may be for a simple invariable element of behavior. To this element the term "reflex" or an equivalent one might be applied, and we might then maintain that the behavior of all organisms is made up of reflexes.[49]

[48] O. Külpe, Versuche über Abstraktion, *I. Kongress für experimentelle Psychologie,* 1904, 56-68.

[49] H. S. Jennings, *Behavior of the Lower Organisms,* 1905, 281 f.

In these examples, one from classical psychology and the other from the type of work which led directly into behaviorism, we find two leaders in their respective fields of inquiry openly denying the possibility of arriving at psychological laws on the basis of observed items alone. The descriptive data of psychology come from two sources: *A,* the physical and social environment, and *B,* the sensory and behavioral processes of a biological organism. The basic assumption in proceeding to the establishment of correlations is that $B=f(A)$. Experimental technique demands, of course, the more or less artificial splitting of *A* and *B* into parts, so that actual correlations are between *aspects* of *A* and *B.* Even the *Gestalt* psychologists, who maintain that their configurations are totalities, are forced to work with parts, otherwise their *A* would have to include sun spots, and their *B,* the religious heresies of an organism's great-great-grandparent. Some aspects of *A* and *B* are less narrowly isolated than others. The degree of abstraction depends on the kind of problem under investigation. But completely unanalyzed *A*'s and *B*'s are scientifically pure fictions. Many aspects of *A* and *B* can be and have been brought under direct observation, and the relations between them are stated in terms of mathematical functions, coefficients of correlation. It would be gratifying if these observed correlations came out with values close to unity, but as a rule they do not. To meet these correlational deficiencies, Külpe and Jennings proposed the development of concepts in which observational material would be supplemented by infiltration of meanings based on inference. Neither the *physiological state* as defined by Jennings, nor the *mental set,* as used by Külpe, is matter of direct observation. They are guesses, assumed factors that

presumably operate somewhere between *A* and *B*. If such intervening factors are called *C*, then the statement of results would read, $B=f(AC)$.

That enormously large part of psychological literature which has been speculative in character deals almost entirely with guesses as to what *C* is like. Some of the guesses have been, not necessarily correct, but at least good, in the sense that they have grown out of and at the same time remained close to the level of observation; others have been, not demonstrably false, but nevertheless bad, for they have done little more than show what stratospheric flights of fancy the human imagination can make.

Strict adherence to two operational principles would serve to keep scientific imagination within palpable boundaries. One of these, to be dealt with more in detail presently, is the firm insistence that the contours of guesses must conform to the contours of the gaps revealed by the observed items. This practice would tie inferences down to concrete data. Concrete data in science are always correlations. Correlations are nothing more than items observed in particular relations. An observed relation is not a force, a power, a will, or a purpose. Therefore inferences should not contain any meaning, especially any hidden meaning, that implies the action of some power or purpose.

THE LOCUS OF PSYCHOLOGICAL HYPOTHESES

The second principle relates to the *locus* of the inferred variable or factor. If it is necessary to postulate an intervening factor, *C*, to account for the behavior of the observed items in *A* and *B*, where should it be assumed that this factor will eventually be found? The answer is simple and in-

escapable. The variable *C* and all of its properties and ramifications must be inside the organism, quite literally inside. Everything outside the organism is *A,* physical and social environment. Since the sensory and behavioral processes of the organism are *B,* the only remaining locus for *C* in the total chain of events is within the organism. This field of action for the properties of *C* is arrived at by the process of elimination, but even if this process were not available, an important further consideration would make it desirable to assume some such location, and then try to discover it empirically.

This further consideration has to do with the elementary scientific principle that even if it were not possible to infer that the immediate determinants of psychological events are within the organism, it would nevertheless be mandatory upon psychologists to place them within a definite region accessible to human observation and verification. In the last analysis scientific truth is always arrived at by observation and experiment. Knowledge that can not be subjected to this test is not scientific knowledge. To the items used in any hypothesis, properties must be assigned which are capable of experimental test, which means that they require a location having spatio-temporal characteristics. The region inside the biological organism has these characteristics, and although the events that take place in this region that are most important for psychology are still obscure, there is no intrinsic reason for believing that they will not eventually be known.

PSYCHOLOGY AND PHYSIOLOGY

In discussing this same problem of the spatio-temporal location of hypotheses, Beebe-Center remarks that "Merely to

state that it (i.e., any psychological hypothesis) is a physiological event strikes me as a superfluous confession of faith." [50] Would that it were a superfluous confession of faith! The present literature as well as the history of psychology presents many instances of hypotheses in which their authors neglect to make any reference to spatial or temporal co-ordinates in terms of which it would be possible to locate the items referred to in the hypotheses. Indeed, many psychologists openly object to any tendency on the part of psychology to move in the direction of physiology. At least four of these objections still find expression in the literature of today.

1. The ancient dislike for science on the grounds that it robs man of his cherished beliefs and values is directed with special vigor toward psychology. Science in general is regarded as materialistic and mechanistic, and if psychology becomes "too scientific," then the human mind will not be distinguishable from the blind impersonal forces of nature. Human nature will become a materialistic nature. This fear springs from a complete misunderstanding of scientific methodology and explanation. The misunderstanding could be easily eliminated if the emotional prejudice were not so strong, but in the face of indiscriminate hurling of words not much can be done; although an attempt will be made in the next chapter to eliminate some of the misunderstanding. Certain psychological procedures are screamed at and called *materialistic* and *mechanistic* until the words retain little more meaning than do such words as *fascist* and *communist* when violently subjected to similar treatment. It would be

[50] J. G. Beebe-Center, The law of affective equilibrium, *American Journal of Psychology*, 1929, 41, 68.

pleasant to dismiss and forget the prejudice, but since it often retards the progress of science it should not go unnoticed and unchallenged. Theoretical psychology can advance only in proportion to its ability to frame its explanatory principles in the language of physiology. Anything that will serve to strengthen this conviction is not yet a superfluous confession of faith.

The three objections which follow are not due to prejudice, nor to any misunderstanding of scientific methodology. They represent instead perfectly intelligible differences of opinion toward points of view that are not basically incompatible, but which receive different degrees of emphasis as the result of special interests and purposes.

2. With all of its imperfections and insecurities, psychology is nevertheless in possession of a much larger body of factual material than is that portion of physiology of which this material is presumably a function. In view of this situation many psychologists feel that it would be much wiser not to bother with physiology, but to proceed with the task of extending the boundaries of psychological knowledge still further. No possible objection can be raised against such a motive, provided it does not include an active disapproval of physiological psychology. Observational and descriptive procedures are the first step in any science, but they are only first steps. Facts acquire more meaning when they are subsumed under larger systems of generalization, but as Külpe and Jennings both tried to make clear, it is these systems that a purely descriptive psychology fails to offer. Only physiology can furnish an explanatory frame of reference for the facts of psychology.

The facts of sensation and reflex action already begin

to find their place in a physiological framework, so much so, indeed, that it no longer makes any difference whether these studies are called psychology or physiology. Because other fields of psychology can not yet bridge the gap is no reason for insisting that their procedures should remain purely descriptive without any attempt at formulation of hypotheses in physiological language.

3. Since true explanation and observed correlation turn out to be one and the same thing, some psychologists, especially the more sophisticated, defend the view that all attempts at explanation, beyond that revealed by the observed material itself, are crude and outmoded. Among the classical psychologists, Titchener was probably the most ardent defender of this attitude. "Science is not called upon to 'explain' anything; description and correlation are the modern —and more modest—representatives of the 'explanation' that an older science looked for and professed to find." With respect to help from outside sources, "it does not occur to us," said Titchener, "to imitate physiology in the presentation of results. . . . We do not lean upon physiology. . . . We feel ourselves independent." [51]

The most recent stanch supporter of this same view, this time a behaviorist, is Skinner. "We may now take that more humble view of explanation and causation . . . wherein, in a word, explanation is reduced to description and the notion of the function substituted for that of causation. . . . In the description of behavior we are interested in the relationships within a regressive series of events extending from the be-

[51] E. B. Titchener, *A Beginner's Psychology,* 1915, 327; also, Experimental psychology: a retrospect, *American Journal of Psychology,* 1925, 36, 322 f.

havior itself to those energy changes at the periphery which we designate as stimuli. We stop here in the regression only because further steps are beyond the field of behavior." Skinner urges psychologists to leave physiology alone, for "behavior may be adequately described without constant reference to its physiological correlates." [52] Similar admonishment is expressed by McGeoch. "The wisdom of foregoing the speculative delights of the nervous system for the world of experiment . . . is at least defensible." [53]

The position advocated by these writers is indeed most eminently defensible. From the point of view of the logic of science it represents, in the last analysis, the only tenable position. The trouble in adhering to it strictly is that psychology is not yet anywhere near "the last analysis." Too many items that would be required to work out a system of correlations with explanatory value are still missing. An explanation turns into a correlation only when the assumption contained in it has been verified by empirical test. If empirical test is impossible, as more frequently than not is still the case in psychology, the explanation is not a true correlation, but a guess.

At the formal or logical level there is no difference between an explanation in which certain items are matters of guesswork and an explanation in which all items are matters of fact, but at the material or empirical level there is all the difference in the world. One *may* be true, the other *is* true. The position of the correlationists is a safe one, but it would

[52] B. F. Skinner, The concept of the reflex in the description of behavior, *Journal of General Psychology,* 1931, 5, 446; also, Drive and reflex strength, *ibid.,* 1932, 6, 34.

[53] J. A. McGeoch, Forgetting and the law of disuse, *Psychological Review,* 1932, 39, 369.

rob scientific work of a good deal of its hazardous delight and of many of its stimulating hypotheses. It would mean that speculations like those of Köhler or Spearman, which helped to construct the framework of theory into which the brilliant discoveries of Lashley fit so systematically, would be thrown out because of their considerable element of unverified guesswork.

4. Speculations of this type, however, are frequently referred to as *brain mythology,* rather than brain *physiology,* and many psychologists insist that the good reputation of their science requires them to observe the amenities consequent upon the indubitable necessity for division of labor in modern science. They prefer to draw a more or less definite line of division between physiology and psychology. All such lines of division, however, are quite arbitrary, and are bound to disappear between adjacent fields like physiology and psychology, both of which are so vitally concerned with the characteristics of living organisms.

In proportion as the experimental work of psychology becomes exact, it will inevitably be absorbed into the more basic discipline of physiology. Moreover, the slur implied by calling the physiology of psychologists brain mythology is generally not warranted. All scientific concepts are *constructions* built up by the rules of logic from the uniformities that are found to exist among the initial data of observation. If the initial data are functions of unknown physiological determinants, as it is only reasonable to assume psychological data to be, then the concepts based on them may be regarded logically as an indication of the formal properties of the determinants. Formal construction of concepts is no less common in physiology than in psychology. The manner in which Sherrington

'constructed' the synapse, which Skinner has analyzed so beautifully,[54] may serve as an example.

THE SYNAPSE

In 1906 Sherrington wrote a sizable volume devoted for the most part to a description of eleven characteristic differences between conduction in nerve-trunks and in reflex-arcs.[55] He located these differences at the surfaces of separation at the nexus between neuron and neuron. These differences established the properties of the so-called *synapse.* It should be noted, however, that the synapse was never observed, nor were any events at the nexus of neuron and neuron recorded, nor was conduction in reflex-arcs open to direct inspection. All of these properties were constructions based on observations which consisted solely in noting certain qualitative and temporal characteristics of end-effects in muscle in relation to application of stimuli. In the left-hand column below and on the next page are given Sherrington's eleven properties of the synapse. In the right-hand column are listed the kinds of observations actually made.

Characteristics of conduction in reflex-arcs, as contrasted with conduction in nerve-trunks:	As indicated by:
1. Slower propagation	1. Length of delay between application of stimulus and appearance of response
2. Marked after-discharge	2. Prolongation of response beyond cessation of stimulus

[54] Skinner, The concept of the reflex in the description of behavior, *Journal of General Psychology,* 1931, 5, 441 ff.

[55] C. S. Sherrington, *The Integrative Action of the Nervous System,* 1906.

Characteristics of conduction in reflex-arcs, as contrasted with conduction in nerve-trunks:	As indicated by:
3. Transmutation of rates of impulse	3. Lack of correspondence between undulations of response and rhythm of stimuli
4. Functional solidarity of elements that compose reflex center	4. Difficulty of grading intensity of response with the grading of stimulus-intensity
5. Prolongation of excitatory phase	5. Appearance of response after a succession of subliminal stimuli
6. Irreversibility of direction	6. Absence of response to stimulation of central end of motor nerve
7. Fatigability	7. Weakening of response with repetition of stimulus
8. Variable resistance	8. Changes in threshold value of stimuli
9. (a) Refractory period	9. (a) Failure of second stimulus to elicit normal response during short interval following first response
(b) Reinforcement	(b) Combination of several responses elicited by a single stimulus
(c) Inhibition	(c) Failure of stimulus, under certain conditions, to produce a response
(d) Depression of function	(d) Temporary alteration of response following injury to nervous system

Characteristics of conduction in reflex-arcs, as contrasted with conduction in nerve-trunks:	As indicated by:
10. Greater dependence on blood circulation	10. Change of response with change in blood supply
11. Greater susceptibility to various drugs	11. Change of response with drugs

In reading Sherrington it is by no means obvious at first glance that the words used to label these eleven characteristics have no definable meaning apart from the experiments cited later in the text. They seem to refer to definite processes in reflex-arcs, and were probably so intended by their author, but critical examination of the text makes it clear that this reference is a type of conceptual reification. The only real data are the observations of stimuli and responses. The rest is guesswork, but guesswork which is grounded in good logic, although the logic is not made explicit. Failure to verify the physiological processes implied in Sherrington's concepts would invalidate not the logic, but the material items suggested by the logic. If, for example, in the case of No. 2, no physico-chemical evidence could be found to give any meaning to *after-discharge,* that particular hypothesis would have to be abandoned, only to give rise to another. Prolongation of response beyond the cessation of stimulation would still be a fact, and would still demand some explanation in terms of neural physiology.[56]

[56] L'hypothèse ainsi renversée a-t-elle donc été stérile? Loin de là, on peut dire qu'elle a rendu plus de services qu'une hypothèse vraie; non seulement elle a été l'occasion de l'expérience décisive, mais on aurait fait cette expérience par hasard, sans avoir fait l'hypothèse, qu'on n'en aurait rien tiré; on n'y aurait rien vu d'extraordinaire; on n'aurait catalogué qu'un fait de plus sans en déduire la moindre conséquence. H. Poincaré, *La Science et l'Hypothèse,* 1902, 179.

MATERIAL AND FORMAL PROPERTIES

If the operational logic by which such concepts as Sherrington's are constructed were made more explicit, it would be easier to evaluate them. It would then be possible to tell more readily the difference between good and bad guesses. To this end it may be helpful to distinguish between what may be called *material* properties and *formal* properties.

By material properties is meant those events which are directly observed. They constitute the initial data of all science. They are the Alpha and also the Omega of science. The items on the right in the above lists are material properties. They are the original data in terms of which Sherrington began his speculations. Any words whose intent is to summarize the items contained in observed correlations perform the service of material properties.

Formal properties are more elusive, for they are always to some extent matters of degree. Material properties are absolute, by which is merely meant that they are as fixed as are the facts which they summarize. If new methods undo earlier observations, then the corresponding material properties are undone. But formal properties are intended, with the help of operational logic, to carry scientific imagination beyond observation. The degree to which they can be given operational definition is therefore a relative matter. If they merely summarize observations in terminology that derives from a different set of linguistic concepts, they may be defined as precisely as are the material properties, for they *are* the material properties expressed in different but synonymous phrases. If, however, in addition to summarizing observations, they also suggest items in terms of which the observations may be

explained, an element of non-operational meaning has then entered in. But this element is not scientific nonsense, for its meaning is governed by the observations. A simple example may serve to illustrate the manner in which formal properties are constructed.

Let us suppose that the following observation is being made for the first time. On an ordinary sheet of typewriter-paper, held with the long side horizontal, a dot is made two inches from the left, and a black circle an inch in diameter, six inches to the right of the dot. A person holds the paper in front of him, closes his left eye, and fixates the dot with the other eye. As he brings the paper nearer, and then moves it farther away, he happens to notice that at about the distance of eighteen inches the black circle completely disappears. He is curious about the thing, finds that his friends report the same experience, so he goes ahead and collects a good many carefully measured observations, until he is finally satisfied that he has an accurate description of the phenomenon. If he now brings his observations together, including specification of stimuli, distances, instructions, *etc.,* and summarizes them by some phrase—he may decide to call the thing *peripheral anopsy*—that phrase then defines a material property. As far as sheer observed correlation goes, here the matter ends.

But our amateur scientist insists on being his own physiologist. He makes the very plausible guess that a certain region of the retina must be insensitive to light. By using his initial measurements and doing some simple calculating, he can make a good estimate as to the location and size of this insensitive region. Moreover, he calls this region the *blind spot,* and he can say exactly what he means by that phrase in

terms of his observations and calculations. And then, finally, since he is a naïve believer in causation, he boldly announces that he is sure the disappearance of the black circle on the piece of paper is due to the blind spot!

The blind spot is a formal property, for it was not observed, but its characteristics were derived from what was observed. It is a sound logical inference, a good guess, a justifiable conceptual reification. It raises a clearly defined question for physiological optics to answer. If no blind spot had ever been found, something else would still have to be found to explain the disappearance of the black circles. The correlationists would presumably accept the material property as a valid datum, but would reject the formal property.

The thesis which these pages are trying to defend is not exactly the opposite of that of the correlationists, but is strikingly different in emphasis. *Material properties are of little importance in and of themselves. The formal properties which the material properties define are the important concern of science.* With respect to the illustration here used, these statements mean that the science of vision would not be especially interested in the disappearance of black circles on a piece of paper, but it would be greatly concerned with the question as to what makes the circles disappear.

It would be a simple matter to extend the concept of blind spot to absurd limits. The hypothesis might be made that perspicacity, which is defined in the dictionary as "acuteness of sight," is in some way related to size of blind spot: ability to penetrate to the core of an argument would be correlated with a small blind spot, slow or faulty penetration with a large one. The physiological guesses might go so far as to suggest that the cortex has its blind spot, and that the size

of this region is correlated with degrees of intelligence. Such speculations of course warrant all the objections that correlationists would bring against them.

MNEMONIC TRACE

In the above list of synaptic properties proposed by Sherrington, those on the left are formal properties. They are defined by material properties, but in venturing a guess as to the region in which they might conceivably be located, Sherrington carries them one step beyond operational definition. If all guesses about unknown neural processes are mythology, then physiologists and psychologists have both contributed to the world's store of fable. Many physiological concepts developed by psychologists are no more fabulous than the synapse. Consider, for example, the concept of *mnemonic trace,* which has recently come again into wide use in literature on memory and forgetting. The trace is a formal concept. It is not yet accessible to direct observation, but its formal characteristics are revealed by a variety of materials in the classical studies of Ebbinghaus and in studies of retroactive inhibition, and more recently by observations drawn from the field of psychophysics.[57]

When in psychophysical work a series of comparison-stimuli, symmetrically distributed about the standard, is pre-

[57] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychologische Forschung,* 1923, 4, 115-176; O. Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *Ibid.,* 1932, 17, 130-177; K. Koffka, *Principles of Gestalt Psychology,* 1935, 423-529; C. C. Pratt, The time-error in psychophysical judgments, *American Journal of Psychology,* 1933, 45, 292-297; Time errors in the method of single stimuli, *Journal of Experimental Psychology,* 1933, 16, 798-815; The law of disuse, *Psychological Review,* 1936, 43, 83-93.

sented to an observer for judgment, it turns out that if the interval between the standard and comparison is more than four seconds, a marked preponderance of judgments in the direction of increase (heavier, louder, brighter, as the case may be) will be given. This fact, as sheer correlational datum, has been known ever since the earliest days of psychophysics, but it remained for Köhler to give it life by treating it as an indicator of the physiology of traces. The preponderance of judgments in the direction of increase means that the effective comparison-value of the standard has suffered a decrease. So much is given directly in the correlations between stimuli and judgments, and whatever word is chosen to refer to this stimulus-decrement may be given operational definition in terms of the correlations themselves. But why the decrement? Here the guess is inescapable that the physiological determinant of the judgment has undergone, during the interval between application of stimulus and appearance of judgment, a decline, a disintegration, a deterioration—the word chosen matters little. Disintegration, if that is the word, would then be defined on the basis of material stimulus-decrement, but its formal aspect would involve an element of guess. The formal shape of the guess would nevertheless be molded by the shape of the initial observations.

This particular instance from psychophysics is only one of many observations which have been incorporated into recent developments of trace-theory. By extending the implications of psychophysics, a field has been cultivated which had lain fallow for years. The facts were there, but not enough had been done with them. Placed within a physiological context, they achieve more lively significance, and they set a concrete challenge to physiology. Where is the trace, and what are its

physico-chemical properties? The more accurately such questions are formulated, the greater the possibility of working out experimental methods designed to find answers to them. The answers to such questions are not likely to appear by sheer accident. The language used in these questions must not of course be taken to imply that the trace is a definite thing. The logic by which the word is defined precludes such naïve reification. Trace is a *formal* concept.

The number of well-established facts in psychology for which physiological correlates are still unknown is very large. Psychology is in a position to construct its own formal physiology without waiting for material physiology to catch up with it. Psychological physiology can not be called mythology unless it contradicts known physiological facts, or fails to conform to the principles of concept-construction contained in operational logic.

CHAPTER VI

The Mystery of Mind

THE RELEGATION of material properties to a position of secondary importance (p. 140) alters no fundamental principle in psychology. Observations are still necessary, but the derived formal properties are what give observations their importance. The difference is a matter of emphasis: the significance of observations is extrinsic rather than intrinsic. Colors are important for visual theory, sounds for auditory theory; uniform reactions to simple stimuli throw light on reflex action; similar responses to different configurations of stimuli contribute to theories of mass action and equipotentiality; results from batteries of tests make it possible to construct theories about general and special factors in intelligence; investigations using nonsense syllables would be trivial apart from theories of memory and learning; the number of errors a rat makes in a maze would have small value if separated from all speculation about learning, *etc., etc.*

RELATIVE UNIMPORTANCE OF INITIAL DATA

The greater importance assigned to formal properties is nothing new. The history of science would undoubtedly show that the great majority of experiments have been designed with this distinction at least vaguely in mind; but if the distinction is too vague there is always the danger that

some line of investigation will run up a blind alley, accumulating results that lead nowhere, piling facts upon facts in the mistaken notion that facts are important in their own right. Clear insistence that "facts for facts' sake" does not represent the whole program of science is difficult to find in the writings of scientists, presumably because scientists themselves are likely to accept uncritically the popular view that science, as contrasted with other kinds of intellectual pursuit, deals only with facts.

The most lucid and emphatic realization of the relative unimportance for science of the initial appearance of things finds expression in the writings of a philosopher, "an ignorant man, almost a poet," as he likes to call himself. Throughout the writings of Santayana frequent passages occur which stress the ephemeral nature of essences. Literary psychologists, poets, and novelists may use human emotions and thoughts as vehicles for the expression of their own aesthetic intuitions; but for scientific psychology these data are merely symptoms, formal indications of the nature of material properties within the organism.

> Scientific psychology is a part of physics, or the study of nature. . . . The only facts observable by the psychologist are physical facts, and the only events that can test the accuracy of his theories are material events. . . . Even in the simplest perceptions on which scientific psychology, or any natural science, can be based, there is an essence present which only poetry can describe or sympathy conceive. Schoolroom experiments in optics, for instance, are initially a play of intuitions, and exciting in that capacity; I see, and am confident and pleased that others see with me, this colour of an after-image, this straight stick bent at the surface of the water, the spokes of this wheel vanishing as it turns. For science, these given essences are only stepping-stones to the conditions under which they arise, and

their proper aesthetic nature, which is trivial in itself, is forgotten in the curious knowledge I may acquire concerning light and perspective and refraction and the structure of the eye.[58]

Among psychologists, Köhler most consistently adheres to the view that initial observations, whatever their source, are mere essences, "stepping-stones to the conditions under which they arise." Even he does not announce a general thesis to this effect in so many words, but the sort of propositions which he makes leave no doubt as to his attitude.

At the very outset (Chap. II) it was proposed to use direct experience as an indicator of those physiological processes which, occurring between the realm of outer conditions and the overt behavior of the organism, are not directly observable at the present time . . . As depending upon physical events *outside* my organism, objective experience leads to the construction of the surrounding physical world; as depending upon physiological events *in* my organism, it gives me hints about these processes. *There is no reason at all why the construction of physiological processes directly underlying experience should be impossible, if experience allows us the construction of a physical world outside, which is related to it much less intimately.*[59]

Students of general theoretical psychology are forced to adopt this position by the very nature and purpose of their work, so that whether they are consciously aware of the position or not makes little difference. No theory can be formulated, no generalization made, which does not leave first observations at least a few steps behind. With individual or applied psychology, however, the situation may be different. As a general rule, sound applications of knowledge can only

[58] G. Santayana, *Scepticism and Animal Faith,* 1923, 252-258.
[59] W. Köhler, *Gestalt Psychology,* 1928, 373, and 60 f.

be made *after* the theories from which they derive have been tested and made into laws. In applied psychology this temporal order of events is often dangerously reversed. The pressure for practical results is so great that many psychologists take a gambling chance with applications, and are eager for more facts as a temporary substitute for dependable laws. Facts may be useful even when they have little theoretical import. It must be obvious to the reader, however, that these pages, except for a few comments later, are not concerned with applied psychology.

EXPLANATIONS IN SCIENCE

The greater importance of formal properties, especially in psychology, is due to two considerations. Psychological descriptions are incomplete. They are the end-term in a series of events, many of which are inside the organism, and they can not be fully understood apart from their temporal antecedents. "Introspective and behavioral descriptions must be supplemented, for completeness, by *physiological*." [60] If the physiological events are not yet open to inspection, they need to be constructed, so that when new methods do uncover them, psychophysiologists will know what to look for. The second consideration follows from the first. Formal constructions, since they supply missing items, have an explanatory capacity which incomplete material properties do not possess. This capacity is implied in the logic of scientific explanation, which may first be considered at an abstract level, and then illustrated by concrete examples.

Let us suppose an ideal case in which five items work to-

[60] E. A. Culler, The law of effect, *Psychological Review*, 1938, 45, 210.

gether in the production of some given phenomenon—*a, b, c, d,* and *e.* If the relations between all of these items are adequately observed, the resulting network of correlations gives a complete explanation of the phenomenon. The *why* is fully answered in the *what* and *how* of the five items. Any further attempt at *why* would be an unwarranted leap into mysticism and magic, for beyond the five items there is nothing scientifically relevant to the phenomenon.

It so happens, however, that in the early stages of investigation only three of the items, *a, d,* and *e* are accessible. These are examined carefully, and their intercorrelations stated as material properties. It is clear to the experimenter that the description is incomplete, for the correlations are imperfect and therefore do not afford an explanation of the phenomenon in terms of *a, d,* and *e* alone. Usually *a, d,* and *e* go together, but occasionally *e* fails to appear, or is replaced by an unexpected *f.* However, on the basis of the kinds of uniformities that do exist between *a, d,* and *e,* it is possible to imagine the action of two factors which, taken in conjunction with *a, d,* and *e,* would fully explain the phenomenon. These factors are given formal definition in terms of *a, d,* and *e,* and are called *x* and *y.* *X* and *y* are formal concepts. The explanation of the phenomenon in terms of *a, d, e, x,* and *y* is an hypothesis.

Later on, new methods bring into direct observation a field in which it is assumed *x* and *y* have their locus. In a search for *x* and *y,* two items, *b* and *c,* are discovered, whose material properties coincide very closely with the formal properties of *x* and *y.* Moreover, *b* and *c* fit into and complete the partial system, *a, d,* and *e.* The phenomenon is now given full material explanation in terms of *a, b, c, d,* and *e.*

Or the new investigations have a different outcome. Nothing resembling *x* and *y* is found; or, *m* and *n*, with properties closely resembling *x* and *y*, are found where *x* and *y* were assumed to be, but these two items seem to have little to do with *a, d,* and *e*. These cases would merely mean that *x* and *y* were bad guesses. A plausible hypothesis would have to be abandoned, and the search for the missing items renewed under the guidance of some other hypothesis.

The logic involved in the construction of formal concepts, or hypotheses, contains a step which to some scientists, particularly psychologists, has always seemed disturbing, even dangerous, and at best very misleading. It will be noted that the properties ascribed to *x* and *y* are derived from the material properties *a, d,* and *e*. But then *x* and *y* are used to explain *a, d,* and *e*. It is like saying that a person is unable to sleep because he is troubled by insomnia, or that a certain drug will make him sleep because it has soporific power. The arguments are said to be circular, and it is a time-honored conviction in science and logic that such circles are vicious. They *seem* to offer an explanation, whereas actually, so it is maintained, they do nothing of the kind.

CIRCULARITY

The aversion to circular arguments is the last stronghold of that faith which believes that science can furnish for any given array of well-established facts a *why* which is more than a mere statement of how the facts go together. In certain quarters the faith still lingers on that the laws of nature are something more than the events in nature themselves. The events *obey* the laws, which can not be the same as saying that the events obey themselves. If the statement that

nature is subject to law really means that nature is subject to nature, then the so-called laws of science explain nothing. They are pompous tautologies, vicious circles.

The majority of present-day scientists, particularly the correlationists, have indeed come to realize that a scientific *why* can be no more than an exact *how,* but the corollary is frequently overlooked, namely, that all arguments in support of any given hypothesis must of necessity be circular.

Let us consider one or two of the examples cited above, especially the intentionally over-simplified one of the blind spot.[61] The disappearance of the black circles on the piece of paper constitutes, at first, the only direct observations, the only known items. These items require further items in order to make a coherent, intelligible system. In the absence of these items, the next step must be a guess, but the nature of the guess is dictated by the behavior of the known items. The properties of the blind spot are derived from the manner in which the circles disappear, but then the blind spot is used to explain their disappearance. They disappear *because* it is characteristic of a certain portion of the retina to blot out part of the field of vision. The argument is circular, and is no less circular or tautological when the blind spot is actually found, for now the correct explanation is given in terms of an observed correlation. The only difference between the circularity of an argument and the circularity of a final law is one of material completeness. The gap in the complete circularity of an hypothesis is filled in by guesswork, whereas the circularity of a law is observationally complete. The *why* of the disappearance of part of the visual field is no more than the *how* of functionally related observations.

[61] P. 139 f.

The unknown items in psychological explanations are beyond any reasonable doubt located within the organism. The guesses as to what these items are like therefore tend to make use of physiological language. If these guesses are to be anything more than wild speculation, they have to be strictly dictated by the characteristics of the known items, which means that their explanatory capacity is bound to be circular in nature. Objections to such circularity, on the grounds that it leads nowhere, have appeared frequently in psychology from the time of faculty psychology down to the present. The following samples could be continued almost indefinitely, if one wanted to search the literature for them.

It would be possible to consider the law of affective equilibrium as a manifestation of an 'affective level.' But would this really be an explanation? I think not. A 'set,' it seems to me, is either an *ad hoc* explanatory concept, an *X* whose entire nature consists of causing the events described by some specific law, or it is a real entity with definite spatio-temporal and qualitative characteristics. If it be the former, it is nothing but an intellectual anaesthetic.[62]

To make the perseveration theory applicable, one must endow it with all of the properties implied by the results and in the absence of knowledge about perseveration this endowment leads at once to circularity.[63]

The fundamental importance of the synapse seems a logical conclusion, yet we must bear in mind when evaluating theories of learning that the properties of the synapse are still entirely hypothetical. If we deduce its properties from the facts of learning, we gain nothing by explaining learning in terms of these hypothetical properties.[64]

[62] J. G. Beebe-Center, The law of affective equilibrium, *American Journal of Psychology,* 1929, 41, 68.

[63] J. A. McGeoch, Studies in retroactive inhibition, VII, *Journal of Experimental Psychology,* 1936, 19, 687.

[64] K. S. Lashley, in *Handbook of General Experimental Psychology,* ed. by C. Murchison, 1934, 473.

These writers are plainly bothered by the circularity involved in concepts which are constructed from the very facts that the concepts are supposed to account for. Their statements are typical of the objections raised by many psychologists today against physiological speculation. Yet the same kind of circularity is also found in physiology. Sherrington's formal properties of the synapse, defined in terms of the material properties, serve also to explain the material properties.[65] The complaint that nothing is gained by hypotheses of this kind is hardly justified by the history of science. On the contrary, a great deal is to be gained by the use of scientific guesses.

The items which science might conceivably isolate and examine are in number like the sands of the sea, and their number increases with every new development in observational technique. This array of items can never be explored in its entirety. Selection is absolutely necessary. But how is the selection to be made? The formulation of hypotheses is clearly one of the most valuable methods for trying to decide in advance what items are worth careful examination. Many writers on methodology insist that science has gained its most important knowledge from this method. In any event, it is hardly true to say that science gains nothing from hypotheses. The circularity of hypotheses has nothing to do with the case, for all logically constructed hypotheses are inevitably circular. Consider the following illustration from astronomy.

Soon after the planet Uranus was discovered, it was found that its observed positions did not permit the calculation of an orbit which could be accounted for by the known motions of Jupiter and Saturn. It was at first thought that the diffi-

[65] P. 135 f.

culty was caused by inaccurate observations, but several years of careful checking failed to eliminate the discrepancy between fact and theory. The Reverend T. J. Hussey, an English amateur astronomer, first made the suggestion that the unaccountable deviations in the orbit of Uranus must be due to the action of a still more distant planet. Professional astronomers eventually took up the suggestion and calculated the sort of orbit an ultra-Uranian planet would have to describe to produce the observed positions of Uranus. For several years the calculated planet was regarded as a reasonable explanation of the motions of Uranus. Could circularity be more perfect? The formal properties of the calculated planet were derived from the material properties, that is, the observed positions, of Uranus, and then these same formal properties were used to explain the behavior of Uranus. Finally, the calculated planet was actually observed in the predicted positions. A material property, now called Neptune, confirmed the accuracy of the formal properties of an unnamed fictional concept.

If astronomers had been unwilling to construct a circular hypothesis with respect to possible bodies which would explain the otherwise unaccountable deviations of Uranus, their search might have gone unrewarded for a much longer time. With the whole heavens before them for inspection, the chances of training their telescopes in exactly the right direction would be almost negligible. But with the aid of well-planned speculations, the motions of Neptune were discovered and followed, and the motions of Uranus then brought within their proper place in the laws of planetary action. When it is said that the motions of the planets obey these laws, the statement must be understood as a scientific tautol-

ogy. The laws are a summary of observed motions. The laws state not *why,* but *that* the motions are what they are.

> What is called explaining one law of nature by another, is but substituting one mystery for another; and does nothing to render the general course of nature other than mysterious; we can no more assign a *why* for the more extensive laws than for the partial ones.[66]

Science is a vast and impressive tautology. Its laws are summaries of observations, its hypotheses involve arguments that are circular. Since its explanations are true only if they can be demonstrated empirically, they explain nothing that is not already known. The mystery which surrounds the life of man is as dark today as it was when man first came out of the jungle, and will be just as impenetrable when the last surviving scientist thinks his concluding thought or writes his final sentence. Science can not transcend the knowledge presented to the five senses, but the mind of man would like to know the *why* of his senses and *why* objects and people behave the way they do. These queries, unless they merely mean *how* things behave, are forever unanswerable.

SCIENTIFIC TRUTH AND PREDICTION

The power of science lies not in its ability to remove mysteries, but in its ability to predict, with varying degrees of accuracy, the course of events in the future. The *how* of things past gives a hint of how things will be in the future. The control thus exercised over nature does, to be sure, rob nature of some of its mystery, but it is not the kind of mystery meant by those who want to know *why* things are as they are. The laws of planetary motion make it possible to

[66] J. S. Mill, *A System of Logic,* 1843, Bk. III, Ch. 12, par. 6.

predict where a given planet will be, but they do not answer the question as to why the planet could not be somewhere else rather than where it is. To say that the planet *obeys* certain laws is only another way of saying that it does what it does. It does not say that the planet does what it does *because* of something or other. The inability to assign any meaning to this kind of *because* leaves the mystery of the universe just where it always has been and always will be, namely, beyond the range of human solution.

From the point of view of operationism or logical empiricism, attempts on the part of science to make predictions involve a contradiction. The only indubitable facts are those contained in the original protocols or observations. At time *t,* under conditions *c,* events *e* were recorded. These facts were all in the past, or if they are being recorded at the present moment, they are just on the point of becoming past. If the observations are repeated, under suitable experimental variation, and uniformities established, the resulting laws are still laws of past occurrences. And yet science boldly applies many of its laws to events still not in existence. Taken strictly, the predicate *true* can apply only to an occurrence which has been found true by direct observation. In this limitation lies the inescapable tautology of strict scientific propositions. A future event does not exist and can not be known by direct observation. Therefore all propositions about the future, whether made by science or by any other system of knowledge, are neither true nor false. They are in this respect meaningless, for the words true and false are inapplicable to them. Yet no one doubts for a moment the truth of a prediction made by astronomers that an eclipse will occur at a certain time and place.

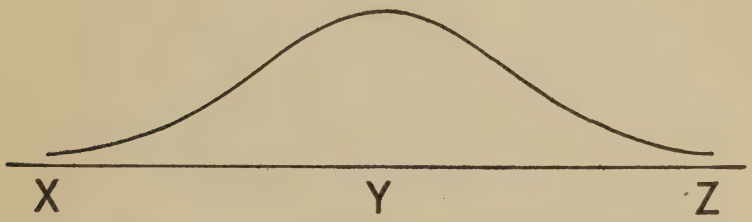
A good proportion of our lives is governed by a paradox, namely, by unquestioned faith and confident anticipation with respect to events, and their effects upon us, which do not exist. The wisdom and truth of these attitudes is taken for granted—even when it is discovered that the logic of prediction offers no scientific justification for such attitudes. Which is wrong, our attitudes, or the logic of science? Neither is wrong—nor right, for in this context *true* and *false* are meaningless words, or, at best, relative concepts, which become absolute only through verbal courtesy, not through empirical demonstration.

The truth of any proposition about some future event can only be known *after* the event has occurred. To ask whether, in the light of *ex post facto* verification, the proposition was true at the time it was made is nonsense. The accumulations of nonsense furnish the basis of the illogical conviction that at the time they are made many scientific propositions about the future are true.

A tolerable way out of this dilemma is found by accepting the pragmatic value of the notion of *probability.* No proposition about the future is either true or false. Every such proposition has merely a certain probability-value. This value comes from the past and is then projected into the future. If certain events, under known conditions, have always occurred in the past, under these same conditions the probability is high that they will also occur in the future. There is nevertheless no way whatever to prove, *i.e.,* to establish the truth of any proposition about the future occurrences of these events until the occurrence has been recorded, by which time the proposition is no longer a prediction.

Truth and falsehood, in relation to predictions, are there-

fore limiting cases on a scale of probability. There is no such thing as absolute truth. It is permissible to say that a proposition is true only if the statement is understood to mean that the probability of its failure to come true is so small as to be practically negligible. Probability-values may be thought of as lying along the line *xyz*. In the direction



from *z* to *y* the probabilities that propositions will turn out to be true become increasingly less, until, in the region of *y*, they have no probability-value at all. From *y* to *x* the probabilities that propositions will turn out to be false become increasingly greater. The height of the curve over *xyz* may be thought of as an indication of the number of propositions at any given point on the scale: in the directions *y* to *z* (toward true) and *y* to *x* (toward false) the number of propositions becomes progressively fewer. Truth and falsehood are not on the scale. Their existence can not be demonstrated. They are approached at the extremes, but are never fully reached.

CAUSAL AND EMPIRICAL LAWS

A distinction has often been made in science between causal and empirical laws. The difference, in relation to the problem of prediction, is really one of degree, not of kind. A causal law means the formulation of observed correlations to which some form of the method of concomitant

variations has been successfully applied. The antecedent terms of the correlations, the independent variables, have been altered or omitted in different experimental series in order to find out the quantitative extent to which the dependent variables are *functionally* related to their correlative antecedents. Observations of this kind allow greater exactness of formulation, and wider application to conditions not identical with those under which the observations were first made, for since the functional interdependence of items is known, predictions with higher probability-values can be made as to what will happen in the case of differing constellations of items.

Empirical laws also represent formulations of observed conditions, but in their case some of the antecedent terms are not subject to control or variation, or some of the items which unquestionably influence the correlations are inaccessible. The method of concomitant variations has limited application to these correlations, so that the probability-value of any generalization of these laws beyond their original conditions is usually low. The initial correlations may have been high, but since some of the items either could not be controlled or were not subject to variation, there is no way of forming a reliable estimate as to what would happen if the constellation of conditions were altered. It may even be impossible to tell whether two constellations are alike. They may appear similar on the surface, but some of the underlying inaccessible conditions may be different. Empirical laws are therefore limited in scope, and generally lower than causal laws in probability-value.

Every psychological law, or correlation, is at best partly empirical. Either the antecedent terms are not subject to con-

trol, or variable intermediate factors are inaccessible, or both —often enough, both. The psychological fields to which the method of concomitant variations can be applied with some success are few in number, and from the point of view of significant practical prediction and application, the least important.

In the field of sensation, for example, the method can be used. The independent stimulus-variables are subject to controlled variation, the correlative psychophysical judgments allow quantification of the sensory dimensions, and the intermediate physiological factors, under suitable instructions to the observer, seem to remain relatively constant. With the end-term of the correlations thus controlled and relatively undisturbed by intermediate physiological factors, series of experiments may be run through which are designed in accordance with the principles of concomitant variations. Predictions in this field consequently have relatively high probability-values. The proposition that sixty-five percent reduction of frequency-differences between adjacent notes would make any theme from Palestrina unrecognizable to an audience of expert musicians would be a safer bet than the proposition that the same audience could tell the difference in musical abilities between the best ten percent of a class, selected on the basis of psychological tests of musical talent, in any good conservatory of music, and any other ten percent of the same class selected at random. The method of concomitant variations can thus be applied, with fair success, to problems in such fields as sensation, also reflex action, perception, learning and memory, emotion, to a few problems in intelligence and reasoning—in short, to problems in which some control of stimulus variables is possible, and in

which the physiological state of the organism can be kept moderately constant. Unfortunately these fields offer the least nutritious and exciting results to a society eager for psychological revelations. Social behavior, guidance of aptitudes and interests, control of public opinion, motivation, personality, abnormalities: to these fields society turns for an extension of intellectual and scientific diet, only to discover, so far, that the nourishment is meager.

HIGHER REACHES OF HUMAN NATURE

The so-called laws of complex forms of human behavior are crudely empirical. The observations on which they are based possess, as a rule, no generality beyond the local circumstance which first revealed them. The reasons are obvious. Between complex forms of behavior and antecedent stimulus-items the correlation is nearly zero. That Wagner scored the third act of *Tristan und Isolde* in Venice and thought out the prelude to *Die Meistersinger* while walking the streets of Paris are interesting bits of knowledge to Wagnerites and students of biography. But to psychologists these facts reveal nothing about the source of Wagner's creative activity. The geographical environment, as Koffka calls the external stimuli that impinge upon the organism, plays a negligible role in complex mental processes.

The Geographical and the Behavioural Environment. On a winter evening amidst a driving snowstorm a man on horseback arrived at an inn, happy to have reached a shelter after hours of riding over the wind-swept plain on which the blanket of snow had covered all paths and landmarks. The landlord who came to the door viewed the stranger with surprise and asked him whence he came. The man pointed in the direction straight away from the inn, whereupon the

landlord, in a tone of awe and wonder, said: "Do you know that you have ridden across the Lake of Constance?" At which the rider dropped stone dead at his feet.

In what environment, then, did the behaviour of the stranger take place? The Lake of Constance. Certainly, because it is a true proposition that he rode across it. And yet, this is not the whole truth, for the fact that there was a frozen lake and not ordinary solid ground did not affect his behaviour in the slightest. It is interesting for the geographer that this behaviour took place in this particular locality, but not for the psychologist as the student of behaviour; because the behaviour would have been just the same had the man ridden across a barren plain. But the psychologist knows something more: since the man died from sheer fright after having learned what he had "really" done, the psychologist must conclude that had the stranger known before, his riding behaviour would have been very different from what it actually was. Therefore the psychologist will have to say: There is a second sense to the word environment according to which our horseman did not ride across the lake at all, but across an ordinary snow-swept plain. His behaviour was a riding-over-a-plain, but not a riding-over-a-lake.

What is true of the man who rode across the Lake of Constance is true of every behaviour.[67]

Whether what is true of the man who rode across the Lake of Constance is true of every behavior is questionable. The behavior of the knee in response to a tap on the patellar tendon can be traced pretty directly to the geographical environment, to the stimulus. Physiological factors intervene, to be sure, but they seem not to shift the response in unpredictable directions. Application of a stimulus to the knee does not cause a movement of the ears and scalp in one man and in some other man a movement to encourage more and better sit-down strikes. Nor is it necessary in psychophysical experi-

[67] K. Koffka, *Principles of Gestalt Psychology*, 1935, 27 f.

ments to find out whether the subjects are Catholic or Protestant, Jews or Mohammedans. The judgments are too closely governed by the geographical environment, by the stimuli. In the case of more complex behavior, however, the situation is entirely different. Everyone is forced to agree with Koffka that the geographical environment represents a remote and unimportant item. With respect to man's ambiguous and involved ambitions, it is obvious that the particular stage upon which he performs them does not, in the majority of instances, "affect his behaviour in the slightest."

The important determinants of man's higher activities are inside of him, which means that to apply the method of concomitant variations to them is still out of the question. It is possible to construct hypotheses about them, but there is no way yet to test the hypotheses. Neither is it possible to test some of the hypotheses regarding simpler activities, but their probability-value is higher because they are constructed from more uniform empirical observations.

An attempt at partial escape from this dilemma is frequently made by applying statistical averaging to large numbers of cases. An assumption is made, for example, that the factors which underlie intelligence tend to go with certain kinds of social attitudes more frequently than with others. Examination of many college undergraduates at Columbia shows, perhaps, that a liberal attitude in politics is found more often among the brighter than among the duller students. But such facts can not be generalized with safety beyond the boundaries within which they were collected. The exact opposite might be found at Princeton. College faculties, which, let us hope, are not too dull intellectually, are notoriously conservative; but it may be that professors do not rep-

resent, in relation to the total adult population, a level of intelligence comparable to the brighter students at Columbia; or it may be a matter of fear of losing jobs; or the factor of age may be important; or Columbia may be a pernicious hot-bed of red radicalism in the pay of Moscow, which contaminates the bookworms but not the athletes; or the bright students at Princeton may enjoy a little spoofing in writing their answers, or their fathers may have threatened to cut off allowances if any crazy ideas got into their sons' heads; or the examiners, in the absence of operational definitions of liberalism and conservatism, may have diagnosed social attitudes incorrectly; or the tests of intelligence may have been statistically a bit unreliable. Who knows?

Or again, the suggestion has been made that both avarice and philanthropy are caused by anal eroticism in infants. The desire to hoard wealth and the pleasure of getting rid of it go back to infantile tendencies to delay defecation in order to accumulate fecal matter and thus heighten the erotic pleasures of anal expulsion. Psychoanalysts defend the hypothesis on the basis of their case histories, which, so far as they go, are undoubtedly reliable enough. But even if one swallow may mean that summer is on the way, a whole flock of case histories can not prove a theory. In order to show causal connection between anal eroticism and philanthropy, it would have to be established that in childhood all philanthropists indulged the pleasure of delayed defecation, that all children who engaged in this practice grew up to be philanthropists, that children who gave no signs of this tendency revealed no philanthropic traits later in life, and that no adult unblessed with large generosity was ever caught as a child not going to the bathroom as soon as he should. Investiga-

tions of this kind have not been made. They require time and patience and devotion, especially since the outcome after years of study may be *nil.* Case histories give valuable hints as to what lines of investigation should be followed, but in themselves they are worthless as far as establishing functional dependence is concerned. Psychoanalysts seem temperamentally unable to grasp this fact.

If incidentally the work of Freud (and others like him) could only be properly judged in this respect, many of the controversies over his importance to psychology would disappear. Freud is what Santayana would call a literary psychologist, not a scientist. Such distinctions are of course relative, and often invidious, but for certain purposes they are nevertheless useful. Freud will be remembered long after the names of most scientific psychologists have been forgotten—and his reputation is fully deserved. But so is that of a novelist like Dostoevsky. Freud is a brilliant artist whose glamorous perceptions and piercing intuitions have held the modern intellectual world enthralled. The tremendous sweep of his imagination has enabled him to see connections where narrower minds see nothing. Whether the connections are *really* there or not, no one knows, not even Freud himself; or if he does, he has committed an unpardonable scientific sin by not revealing to the rest of the world the secret of his knowledge. Many generations of psychologists will spend their lives trying to translate the poetry of Freud into the prose of science. If in the end they fail, the fame of Freud will still be undiminished. For in addition to stimulating scientific inquiry, he has greatly deepened the art of the novelist and the dramatist, and this contribution will never be hurt by the inroads of negative criticism.

No attempt should be made to evaluate Freud as a scientist, for he is not a scientist in any strict sense of the word. His works should be read with the same attitude that one might adopt towards philosophy: stimulating, fascinating, individualistic, full of practical suggestions and hints for theory, but almost wholly lacking in the logical and experimental demands of science. When lesser men try to follow in the footsteps of Freud, they sink to their necks in mud. Freud is an exception, a genius. In spite of the fact that the scientific world could ill afford to lose them, such men are dangerous. The slow grind of science is safer and more conducive to genuine advance. If it is not so, then the history and logic of science are deceptive, and along with the pages of this book, should be thrown into the discard.

Since so many important psychophysiological variables are inaccessible, is there any point in trying to make guesses about them? It is a fair question, but not an easy one to answer. Inaccessibility is, of course, a matter of degree. Many important pages of physiological psychology have already been written, many of them in the last decade, some of them earlier. The work of men like Pavlov, Sherrington, Wever, Bray, Cannon, Crozier, Adrian, Lashley, Culler, Goldstein—to cite a few names that come first to mind—illustrates the possibility of bringing psychophysiological processes out into the open. The new book of Stevens and Davis is an impressive example of the manner in which investigations that only a few years ago were regarded as pioneer efforts have already gone a long way toward moving a whole field of psychology over into physiology, or rather, fusing two relatively disparate fields into an indistinguishable whole.[68] Certain

[68] Stevens and Davis, *Hearing*, 1938.

branches of psychology have crossed the threshold into the region where formal logical properties are being converted into material biological properties. And a new generation of psychologists, trained more in physiology than in psychology, will soon cause the earlier work in these fields to lose all but its historical value.

Yet it can not be denied that for the most part these studies throw little, if any, light on the determinants of man's higher activities. These determinants are still completely mysterious. Speculation today comes no closer to their material nature than did the philosophy of Plato or the theology of St. Augustine. Does such speculation have any value? Again, it is a matter of degree.

If material psychological properties, no matter how dubious they may be as empirical laws, show any decent sort of regularity in their connections, the formal properties derived from them must, as Köhler insists, indicate in some measure the nature of their underlying determinants. Studies in learning, memory, forgetting, affectivity, and even reasoning, especially when forced into the methodology of psychophysics, may serve as examples. Many of the data in these studies hold up under controlled observation fully as well as did those from which Sherrington deduced the nature of conduction in reflex-arcs. Certain regularities in what is called retroactive inhibition, for example, are so stable, within the limits of their experimental setting, as to warrant the construction of hypotheses in the language of physiology. The inevitable circularity involved in such construction is no drawback. The important point is that the data upon which they are based are so reliable that psychophysiologists can not afford to ignore them. Data from other fields, however, are so lacking

in stability that it is small wonder that physiologists tend to think of them as useless.

If physiologists who are not unreasonably prejudiced against psychology, as many of them unquestionably are, find certain data useless for hints regarding related physiological studies, then it would be wiser for psychologists to leave these data strictly where they belong—at the level of description. Any attempt to use them as reliable guides in the construction of hypotheses is worse than useless, for it stirs up harmful controversies, bitter prejudices, and creates warring camps that have no clear idea what it is they are fighting about. But it was argued earlier in this chapter that material properties, as sheer descriptive data, are relatively unimportant in their own right. And so they are. Which is intended to say just what it implies, namely, that for scientific psychology the descriptions of man's higher activities are relatively unimportant. If valued for aesthetic reasons they are found best not in literary psychology, but in Shakespeare and Goethe.

It may seriously be doubted whether the whole mass of speculation by psychologists about the higher reaches of human nature has any more scientific value than that contained in the gropings of common sense and the insight of artists. Scientific knowledge and the knowledge of common sense have the same origin, and for long periods of time in the history of any subject they go together and are nearly indistinguishable. They are confronted by the same world of experience and must initially record very much the same facts. The first difference appears as one of degree. Science begins to sort its facts out and classify them according to some convenient set of criteria. The facts of common sense tend to re-

main in no particular arrangement. This difference soon leads to a more basic distinction, a difference in kind rather than degree.

Classification of facts, say on the basis of similarities and differences, is the beginning of the method of concomitant variations. Certain facts clearly go together, others just as clearly do not. This is a simple observed correlation. From this point on, science and common sense tend to part company. Uncontrolled observed correlations are not enough, for as soon as some previously observed connection fails to operate according to expectation, then the whole system of correlations is called into question. The notion of *causal* connection, however crudely defined, leads to *experimental* observation. The course of nature is artificially interrupted, tampered with, and made to conform to the demands of the laboratory. To know a thing scientifically means to understand the conditions which underlie a thing and the quantitative influence which these conditions exert. This kind of knowledge is rare in common sense, for common sense performs few experiments.

APPLIED PSYCHOLOGY

Knowledge of human nature, as that phrase is commonly understood, even that possessed by psychology, is still based largely on observations whose causal connections are speculative, dubious, and extremely unreliable for purposes of prediction. Too many determining variables can not be experimentally isolated. The very dislike, often expressed in the form of reasoned arguments, which some psychologists acquire toward laboratory procedures, is often merely defensive, and thinly conceals a counsel of despair. It is unfortunately true that much of human nature still eludes experi-

mental strait-jackets. Why not in all honesty let this fact be fully recognized, instead of allowing the public to suppose that in some miraculous way psychology has cleared up the mystery of mind?

The self-knowledge that society needs so badly is still a long way off. Certain portions of it exist at a descriptive level, but it is woefully incomplete and unsystematic. "A string of raw facts; a little gossip and wrangle about opinions; a little classification and generalization on the mere descriptive level; a strong prejudice that we have states of mind, and that our brain conditions them. . . . This is no science, it is only the hope of a science." [69]

Every applied psychologist should memorize these words of William James. The proof that is needed before a scientific application can have high probability-value all too frequently is lacking in psychology. Yet there is a flourishing activity in applied psychology, boldly proclaiming that it can cure society of its ills. According to American standards these activities may justify themselves if people are willing to pay for them, but they are seldom justified on scientific grounds.[70]

No proof can be furnished that the science of mental healing, as practiced by psychoanalysts, for example, is any better or, fortunately, any worse, than Christian Science. The treatment of maladjustments is more of an art than a science. If abnormalities are treated successfully by men who know nothing of scientific psychology, that fact alone disturbs the assumed correlation between scientific psychology and suc-

[69] W. James, *Psychology: Briefer Course,* 1892, 467 f.

[70] The depths to which professional psychologists can go in their proselytizing of a gullible public may be seen in a new book by H. C. Link, *The Rediscovery of Man,* 1938.

cessful treatments. A man or woman who combines a keen and sympathetic insight into human nature with a love for his fellow creatures and a strong desire to help them can probably do fully as much to pull people out of depressions or correct their delusions as any scientific psychologist; and if he could change the world for these people, present them with large bank accounts, elevate their *IQ*'s, and make them feel as successful as Mussolini, Toscanini, or Di Maggio, he could do wonders for them. Unprofessional friends of man, mothers, fathers, brothers, sisters, and relatives, as well as clinical psychologists, educators, psychoanalysts, teachers, lawyers, rabbis, ministers, and priests, are many of them doing everything in their power to relieve the mental suffering of others. Some of them succeed and some do not. What is the reason for their success or failure? It may be religion, it may be sympathy and love, or it may be a little science; or a mixture of all of these. Until the answer is fully known, the secret of their art remains a mystery.

If there were some special scientific knowledge required for the control of human beings, Hitler would still be an obscure house painter. Leaders of men are not aware of any psychological laws, nor would they be helped by knowing the contents of all the textbooks on applied psychology now in existence. The same would not be true for mechanical, chemical, radio, sanitary, medical, surgical, dental, mining, aeronautical, or electrical engineers. Their training and study, and their textbooks, are indispensable. The influence which these engineers exert over their special objects of interest is not an art or a gift, but a studied science. These statements are platitudes, but they need occasional restatement in order to place psychological engineering in its proper perspective.

In those professions demanding ability and range of knowledge, the conditions which tend to push the ablest men to the top are too imperfectly known to permit of their formulation in psychological laws. In the absence of such laws, the conditions nevertheless continue to operate; the selective factors are there even if they are not well known. Three of these factors are fairly certain and for all that psychological engineering can say to the contrary, they may be the most important factors: the possession of the required knowledge, the continuation of the interest and enthusiasm which led to its acquisition, and, to use a trite phrase, but one which refers to an obvious factor of momentous importance, a pleasing personality. The second and third factors are unfortunately the least subject to improvement through psychological engineering, Dale Carnegie to the contrary notwithstanding, but their presence in an individual can be well enough ascertained after sufficient acquaintance by any person of average intelligence.

EDUCATIONAL PSYCHOLOGY

In the teaching profession, the same three factors are important, plus two others—a genuine desire to teach and the ability to communicate knowledge. What the intercorrelations between these factors may be need not concern us in the present context. Good teachers in all times and places have had these qualities in varying high degrees, and bad teachers have not been so fortunate. They are qualities that lie pretty close to the surface, and are as well understood and perceived by common-sense methods as by any other; and any possible improvement in those qualities relating to personality and contagious enthusiasm will probably be effected about

as well by letting nature take its course as by applying any assumed laws of psychological engineering. The effrontery of educational psychology in setting itself up as a Moses to guide our educational system into a promised land has taken on astonishing proportions and has taken in practically the whole of elementary and secondary public school instruction in the United States.

The basic assumption of educational psychology seems to be that there are psychological laws which prove that one method of instruction is better than another. Now it may very well be that some methods are better than others. The difficulty comes in proving it, or in demonstrating that good teachers are not as a rule aware of the difference, or at least do not unconsciously put it into practice, or that bad teachers, if they were taught the difference, would be able to carry it out effectively. If the proof is in the pudding, where is the dessert? Why do college instructors not moderate their complaint that students coming up from secondary schools are no better prepared than they ever were? If educational psychologists reply that the fault is economic, that the teachers are not good enough to profit by the instruction in methods which they patiently accept during summer school sessions, that same suspicion has been entertained, although not proved, by many people who have known nothing about educational psychology. If educational psychologists really believe that their good intentions are defeated by economic obstacles, they might turn politically to the left, or campaign for more and better teachers to be bought with money taken from the budget for battleships, instead of encouraging more and larger summer sessions for teachers who can not be improved anyway.

Emphasis on the importance of methods of instruction easily becomes an obsession, and too often tends to place method above subject-matter. The *how* of instruction takes precedence over the *what.* The bizarre lengths to which the fetish of methods can go may be seen among music teachers. They proclaim the unexcelled virtues, for example, of the piano methods of Leschetizky, or Matthay, or Schnabel, and their pupils give so much energy and devotion to the manner of playing that they have little time left to know or care much about what they are playing. Between a mediocre performance of a masterpiece and a masterful performance of a mediocrity, their choice is likely to fall on the latter, not so much because of box-office managers or the low taste of the public as because they know too little about masterpieces and too much about performances. If half the time these pupils spent on methods were given over to finding out more about music itself, more good than harm would result. In fact, it is difficult to see how any harm could come of it, for probably no one would be able to detect any difference in the performances, whereas the type of music played might improve perceptibly.

It is appalling to think of the large number of music students who learn to play pieces with admirable skill and good taste, but who have been taught next to nothing about the truly great literature of their instruments. The amount of first-rate music for any given instrument is not large, even for the piano or the violin. Anyone who can play the works of second-rate composers like Grieg or Dvorak has already had time enough to know a good portion of the world's best music. Yet there are plenty of players who can give an excellent performance of Dvorak's *Humoresque* for whom Bach is little

more than a name. Emphasis on method has left too little time and inspired insufficient desire to learn all there is to know about fine content. It is a common remark that amateur musicians often know more about good music than do the professionals.

The same danger exists with respect to instruction in American public schools. Teachers are led to believe that the amount they know is less important than the methods they use for imparting their knowledge, whereas, in the absence of scientific knowledge about the respective merits of different methods, it surely would seem wiser to gamble on amount rather than on method. If teachers must be driven into summer school sessions, let them take courses in their own subjects rather than in educational psychology. The teacher of mathematics who passes a course or two under a leading mathematician will almost certainly know more mathematics as the result, but if he passes a course in the psychology of mathematics, he will be given a certain number of credits for a course in the psychology of mathematics, and possibly a raise in salary.

The procedure which is rapidly fastening itself upon secondary education demands that before a teacher may give instruction in his specialty, he *must* have so many credits in educational psychology or in the psychology of his subject-matter.[71] The time spent in earning these credits could far more profitably be spent in learning more about his own specialty. We do know that people can increase their knowledge, but we do not yet know that educational psychology can teach

[71] In this connection the interested reader should not fail to consult the anonymous article, I didn't have a teacher's license, *Harpers*, 1938, Feb., No. 1053, 291-297.

people to teach. For all we know, it may be that only God can make good teachers.[72]

IN DEFENSE OF PURE SCIENCE

All of the above statements and illustrations with respect to applied psychology are intentionally a bit extreme—the sort of exaggeration a person indulges in who knows there is a good deal to be said in favor of the point of view he is trying to attack. But the exaggeration will do no harm, for there are certainly more psychologists in this country who are willing, in response to some query, to reply, "Psychology has this or that to say on such and such a subject," than there are who reply, "Psychology has little or nothing to say." Every field of human activity involves psychology, and in every field there is someone who looks eagerly to psychology for assistance: advertiser, business man, clergyman, doctor, employer, faker, gambler, henchman, interviewer, jailer, knave, lawyer, manager, nurse, orator, priest, quack, reformer, salesman, teacher, umpire, vampire, writer, yeggman, and zealot. The

[72] I once gave a course in educational psychology in a conservatory of music. The institution was obliged to offer the course, and I needed the money. So everything was all right, except my conscience. It soon struck me that the whole thing was pretty silly business. The bright youngsters who were full of music to their finger-tips and who were eager to get started as teachers were going to be good bets whether or not they listened to me or read their assignments; the others were too bored or dull to profit by either. My conscience was partly saved by starting frequent arguments about the relative merits of different composers and exaggerating my own prejudices in order to see how well the students could defend their own. The shocking thing to me was that some of the students were completely unfamiliar with some of the greatest masterpieces in music, yet they were being forced to spend their time in a course on educational psychology learning to teach what they did not know.

pressure on psychology for practical results comes from all directions, and is difficult to resist.

The ultimate sanction for psychology, as for every other science, is of course practical. If science were good for nothing, it would deserve oblivion rather than financial and moral support. That in a country where requests for financial help for any activity are generally met with the question "What is it good for?" the support of scientific activity should be so generous and widespread indicates that science as a whole does meet the demand for practical results. Science for science' sake is a noble purpose, but no purpose can be high enough to ignore the market-place.

The most practical results, however, are not always the most immediate. This is especially true of a young, complicated, and very confused discipline, like psychology. It is often impossible to tell what results will some day be most useful. To insist upon quick returns might steer research away from a course which would eventually yield far more important and practical applications than anything supplied by the pressure of a narrower utilitarian demand. Psychologists would do well to support the slogan, science for science' sake, in an effort to make clear to a practical-minded public that in the scientific study of human nature haste makes waste. Defense of such a slogan, however impossible it may be as an ultimate ideal, would tend to restrain premature application and teach the public not to expect the impossible from psychology.

The public has been told altogether too much about the so-called laws of psychology. The work of all science suffers to some extent from popularization and over-simplification, but psychology most of all, for since every man thinks he

knows his own mind, he assumes that he can understand psychology and judge adequately every report which he reads in the press. It is no credit to the profession of psychology that so many of its members rush the results of their 'research' into the popular press. Yet the fault is not wholly theirs, for the institutions which support them expect and urge quick popularization. The universities of the land are rapidly losing all dignity and sense of proportion in their eagerness for popular favor.

Time was when the reputation of a scientist was confined largely to the small body of experts who could understand and evaluate his work. Now the scientist feels, in too many cases, that the security of his university appointment is largely a function of his popular fame. Many universities employ publicity men whose job it is to ferret out novel and exciting work being done by members of the faculty. This work is then written up in the form of dramatic and simplified science news items, rushed to the daily press in time to beat the deadline, and, if possible, to beat the arrival of similar items from rival institutions. It is small wonder that professors get the notion that it would be a good idea to meet the publicity men considerably more than half way.

The discovery of the laws which govern human personality, if indeed they can ever be formulated precisely, is a matter for the future. Tentative theories and partial bits of evidence belong in professional journals, not in the public press. Premature simplification and dramatization create in the public mind the erroneous impression that at last psychology has solved the mystery of mind; that scientific knowledge is available for application to modern social problems.

If results of research can be rushed hot from the laboratory

to the public prints, it must mean either that they have not been scientifically formulated or that they misrepresent their original complexity, for the unavoidably involved language of science can not be easily and quickly translated into the language of the street. Helmholtz, in preparing his popular lectures, had to work hard to translate his ideas into language which people unacquainted with physics could understand. The future Helmholtz of psychology, who will not be a university publicity man, will have an even harder task. The intricate networks of psychological intercorrelations, when expressed in the physico-chemical language of physiology, will be unintelligible to the layman. A curious claim of many psychologists, especially the dynamic psychologists, is that their work remains true to life, close to the freshness and likeness of untampered-with human nature. If it does, then it is not science, and to let the public suppose that it is represents a claim that falls considerably short of the scientific ideal of exactness.

The boundaries of scientific knowledge about human nature have not been pushed very far beyond the central point of a vast circle of ignorance, opinion, and mystery. Whatever literary psychology may have to say about the dim and dark regions beyond the frontiers, scientific psychology is, or should be, silent. The greatest practical service applied psychology could render to itself, to the science of mental and social phenomena, and to the public at large, would be to learn where dogmatic pronouncements should give way to silence.

The world is full of rash opinions, violent creeds, and the imperious commands of ignorant little men with large authority. The force of irrational doctrine is powerful, so that

even intelligent men, who in matters of opinion and creed should be as nearly agnostic as possible, find it hard not to take sides, not to be influenced by special pleading. We live in an age of faith and crusades, of dying for causes. To this sorry state of affairs the mental and social sciences can as yet bring little sure help. The truth may make us free, but until then psychology can advance the cause of truth best by refusing to lighten our present darkness with the false glare of premature and uncertain pronouncements.

Psychologists need more than any other group of scientists the protection of cloisters, an atmosphere of detachment and leisure. They can be most useful to society by staying in their laboratories and libraries, there to remain until they can come forth with reliable predictions and well-tested applications. Those who are in the midst of earthquakes and battles do not discover the laws of motion or the reasons why men fight.

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